

Wenner-Gren Foundation for Anthropological Research

Anthropological Perspectives on Ancient Trade

Author(s): Robert McC. Adams

Source: *Current Anthropology*, Vol. 33, No. 1, Supplement: Inquiry and Debate in the Human Sciences: Contributions from Current Anthropology, 1960-1990 (Feb., 1992), pp. 141-160

Published by: [The University of Chicago Press](#) on behalf of [Wenner-Gren Foundation for Anthropological Research](#)

Stable URL: <http://www.jstor.org/stable/2743942>

Accessed: 14/09/2010 10:16

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=ucpress>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



The University of Chicago Press and Wenner-Gren Foundation for Anthropological Research are collaborating with JSTOR to digitize, preserve and extend access to *Current Anthropology*.

Anthropological Perspectives on Ancient Trade¹

by Robert McC. Adams

THE WORD "TRADE" comprises many shades of meaning and has had a curious history. At the risk of oversimplifying, it first appears in Middle English, apparently having been borrowed from Middle Low German or Middle Dutch, with the meaning of a path or beaten track. Modern English "tread" and German *treten* preserve this usage in cognate form. Then, by the middle or later 16th century, the meaning broadens to include a habitual course of action or the practice of some occupation. Our word "tradesman," tending to distinguish the practice of a craft from a professional or commercial calling, may be a derivative of this stage in etymological evolution. The principal modern meaning of the word, the act or business of exchanging commodities by barter or sale, appears later still. Thus, to the extent that the practice of trade and commerce now is associated with innovative, risk-taking, profit-motivated, entrepreneurial behavior, the word has swung around until its connotations are almost diametrically opposed to those that were present originally.

This shift of meaning occurred, of course, as English overseas commerce gradually moved into the position of world supremacy it had attained by the mid-19th century. But does that conjunction imply that the association of entrepreneurship with the reciprocal flow of goods and services is a wholly recent one? Such is the thrust of much

of the work of Polanyi (1957) and his collaborators, devoted to arguments for institutionalized reciprocity and redistribution in primitive chiefdoms and states as opposed to commercial exchange carried on through the medium of the market. Their work has had a wide acceptance by culture historians and archaeologists. Its stress on the need to avoid becoming prisoners of our categories, to consider radically different organizing principles than any with which we are familiar, is undoubtedly a useful and necessary one. But it is at least a matter for debate whether in any society, ancient or modern, human perceptions, beliefs, and behavior have ever compartmentalized in full conformity with either Polanyi's categories or opposing, mercantilistic ones. The analytical separation between "administered" and "market" trade may, in other words, lack a cognitive or behavioral equivalent. As Parsons and Price (1971:187, 189; cf. Salisbury 1968:121-22) have suggested, the shifting intersection between the two is probably more critical.

A corollary to the Polanyi position, adapting one of its central precepts to the work of prehistorians, has been put forward by Renfrew (1969:152):

The full-time trader, a non-producing middle-man deriving his livelihood or sustenance entirely from these exchanges, is often the product of more recent and more advanced societies. For this reason ethnographic parallels have to be used with extreme care, and comparisons between prehistoric colonisations and trade and those of European commerce in the world during or since the Middle Ages are of doubtful validity.

One can hardly disagree with the literal meaning of this statement, but I must question it as a prescription for research. To begin with, it would surely be unfair to assume that the burden of proof lies entirely on those advocating the similarity of ancient and modern economic motivations and structures, rather than at least in part on their opponents. A sweeping, rigid distinction between ancient patterns and post-medieval, European-influenced ones involves a blindness to the biases and deficiencies of virtually all of the ancient data that has come down to us, whether archaeological or textual. Entrepreneurial behavior, in particular, is characteristically nonhabitual and virtually impossible to aggregate. It sometimes eludes identification

ROBERT MCC. ADAMS is Professor in the Oriental Institute and the Departments of Anthropology and Near Eastern Languages and Civilizations at the University of Chicago (Chicago, Ill. 60637, U.S.A.). Born in 1926, he was educated at the University of Chicago (Ph.B., 1947; M.A., 1952; Ph.D., 1956) and has been a member of the faculty there since 1954. He was Director of the Oriental Institute from 1962 to 1968. He was Annual Professor at the Baghdad School, American Schools of Oriental Research in 1966-67 and Resident Director in 1968-69. He has just completed a four-year term as Dean of the Division of Social Sciences at the University of Chicago. His research interests are Middle Eastern cultural and historical ecology, archaeological field studies of ancient settlement and irrigation patterns, and comparative studies of urban civilizations. His publications include *City Invincible: An Oriental Institute Symposium*, co-edited with C. H. Kraeling (Chicago: University of Chicago Press, 1960); *Land behind Baghdad: A History of Settlement on the Diyala Plains* (Chicago: University of Chicago Press, 1965); *The Evolution of Urban Society: Early Mesopotamia and Prehispanic Mexico* (Chicago: Aldine, 1966); and *The Uruk Countryside: The Natural Setting of Urban Societies*, with H. J. Nissen (Chicago: University of Chicago Press, 1972).

The present paper, submitted in final form 18 1 74, was sent for comment to 50 scholars. The responses are printed after the text and are followed by a reply by the author.

¹ An earlier version of this paper was read in a Colloquium on Current Themes in Anthropological Theory at Harvard University in December 1972. I am indebted to Lloyd A. Fallers for numerous criticisms and to Eric Hamp for a discussion of the etymology of the word "trade."

even by a sophisticated contemporary observer, and can only prove still more elusive (if not virtually irrecoverable) in the fragmentary records of routine transactions studied by the ancient historian and in the material remains uncovered by the archaeologist.

The denial that ancient institutions have ethnographic parallels on the grounds that the latter are distorted by European influences also tends to provide a license for conjecture that cannot be supported adequately by exclusively archaeological or archival data. Why should European influence profoundly alter indigenous economies from their first moment of contact, and in ways qualitatively as well as quantitatively different from the effects of earlier cultural contacts? Even the so-called pristine civilizations were not entirely autochthonous in their origins, but arose as a consequence of important interactive processes as well.

Briefly to restate the case, I suggest that ancient trade is often too narrowly identified with habitual patterns in the movement of goods. It is this in part, of course, and that part happens to be neatly coterminous with the kinds of evidence we can predictably recover and unambiguously interpret. Thus "trade" and "diffusion" become almost equivalent terms in much of the archaeological literature. As a process of social differentiation and interaction, however, trade ramifies much more widely into the institutional structure of a society than is suggested by any definition of diffusion. With particular reference to the ancient Aegean, Renfrew's previously cited article is virtually alone in noting this difference and stating its effects upon the study of sociocultural change.

The more entrepreneurial aspects of trading activities belong with purposive innovation and other forms of goal-motivated behavior. As Erasmus (1969:38) has noted, cultural evolutionists are not familiar with these concepts; they tend to deal instead with gradually evolving group adaptations that are explained in "systemic," situational terms, and in consequence they are unprepared to face consciousness as an agency of change. Perhaps for the same reason, they are also generally unprepared to explain, and often even to recognize, the sudden, perplexing, violent shifts in institutions, values, and behavior that stem from it. Yet students who can employ the fuller written records and absolute chronologies available for later historic periods must confront abrupt, directed, and frequently fundamental change at every turn.

It may be argued that any reference to diffusionism is the invocation of a Straw Man. At the level of theory, it is true, the grand-scale diffusionistic schemes of an earlier generation of anthropologists have long since ceased to be important stimuli for new research. The reaction that arose against them is, however, still with us. And concern for developing a "systemic," ecologically oriented approach to culture history has tended to focus attention on very small-scale, local patterns of adaptation that assume a negligible role for external influences. Such is the clear implication of Binford's (1964:440) dictum that "changes in the ecological setting of any given [sociocultural] system are the prime causative situations activating processes of cultural change." It is made considerably more explicit by Longacre (1970:10), who speaks of "the importance of ecology for explanation in contrast to diffusion" as one of the two most important trends in current archaeological research in the Southwest.

Julian Steward's probably was the most decisive voice in the widespread, now nearly universal, acceptance of ecological setting and sense of process as the proper framework for archaeological and culture-historical analysis. He provided not merely powerful programmatic statements but also one of our small handful of classic empirical studies on hunting bands. But he must also bear at least

part of the responsibility for the fact that, in Willey's (1971:117) words, the field "turned away" from studies in the broad field to which diffusion is a password, in spite of their "crucial implications" for an understanding of sociocultural change. Steward's oft-quoted general statement (1955:182) on the subject of diffusion makes this two-edged effect clear:

The use of diffusion to avoid coming to grips with problems of cause and effect not only fails to provide a consistent approach to culture history, but it gives an explanation of cultural origins that really explains nothing. Diffusion becomes a mechanical and unintelligible, though universal, cause, and it is employed, as if in contrast to other kinds of causes, to account for about 90 percent of the world's culture. One may fairly ask whether each time a society accepts diffused culture, it is not an independent recurrence of cause and effect.

Insofar as it draws attention to the processual aspect of diffusion, his view seems an incontrovertible response to the excesses of the early diffusionists. But there are overtones in the last sentence ("accepts . . . independent") that suggest an unwarranted insistence on the autonomy of the borrowing act, on the independence of choice of the borrowing group, and on the essential isolation of the borrowing process from a broad historical and interactional setting.

Steward's reaction was directed, of course, against an earlier preoccupation with the worldwide tracing of cultural traits, disembodied both from their institutional structures and from the processes by which they spread and were accepted. But the issue is no longer—and probably never should have been—one of the primacy of either disembodied diffusion or autonomous local invention and development. Both social change and social continuity require interactive processes, with the significant interactions in some respect confined to single communities, in others to multiple groups in time-ordered settings, in others to whole regions, in still others to interregional contacts whose historic role was far out of proportion to their limited scale and frequency. The critical task is to establish which types of interaction were linked with which institutional forms and which shifts in interaction were related to other, decisive changes in social scale and complexity.

As Willey's aforementioned review goes on to observe, in discussing diffusion "most of us still come down to very impressionistic resolutions of specific cases." There is currently, to be sure, a major resurgence of interest in the theme of interaction, sometimes over greater distances than can be readily comprehended within middle-range bridging concepts such as "regional mosaics" and "symbiotic regions." Willey's assessment of the subjective character of the field as it exists at present thus serves to indicate how far there is to go to overcome its deficiencies.

The elusiveness of studies of diffusion, their incapacity heretofore to lead us to an orderly series of testable propositions, is not some special limitation of those in the New World. Renfrew (1970:207–8) for example, has commented on the prevailing inattention to diffusion as a process in terms very similar to those of Willey for the New World:

Diffusionism has been the curse of prehistoric archaeology for twenty years. Admirable in the twenties, when given new and systematic expression by Childe, it has subsequently hardened into sterile dogma. . . . It is the lamentable truth that there has been virtually no consideration for European prehistory as to how the mysterious process of diffusion actually works.

It is a critical limitation of archaeology, particularly when not given powerful assistance from historical sources, that it remains tied to the spatial distribution of imperishable objects or nonartifactual materials whose temporal sequence is seldom more than very roughly defined. Questions

normally asked by archaeologists about trade and diffusion have been those which they can (or think they might be able to) answer directly and incontrovertibly from their data. But we must ask whether such questions are the important ones for an understanding of human society.

The diffusion of the quite limited and unrepresentative congeries of artifacts and materials surviving for the archaeologist to recognize is merely one end product of complex patterns of interaction, almost all of them involving reciprocal exchange and almost all of them proceeding simultaneously on many levels. One can provide a meaningful analysis of spatial patterns provided by the introduction of automobiles, agricultural improvements, and other technological advances into southern Sweden (Hägerstrand 1967), the diffusion of hybrid corn in the U.S.A. (Griliches 1960), or the spread of mechanization in mid-19th-century reaping (David 1966), but such studies are possible only because the wider socioeconomic setting in which they occurred is relatively well understood. The prior task, for archaeology and culture history concerned with more remote times and places, is to work out the reciprocal exchanges involved in maintaining and defining those wider socioeconomic settings.

CURRENT STRATEGIES IN THE ARCHAEOLOGICAL STUDY OF TRADE

Ultimately, the end of the era of "impressionism" in archaeological studies of trade and diffusion is likely to require substantial advances in analytical techniques. However, that is only a necessary and not a sufficient condition. It is at least equally important to broaden the conceptual base for the interpretation of new field and laboratory data bearing on these themes, for the kinds of archaeological data now available constrain the research strategies currently being followed to maintain a dangerously high degree of consistency. What is needed in order for this broadening to occur is a much more substantial awareness of ethnohistoric, historic, and ethnographic studies of trade that are already responsive to a richer and more varied series of research paradigms.

One ongoing stream of research may be characterized as seeking more to demonstrate the fact of cultural contact or diffusion than its extent or implications for understanding subsequent institutional change in the societies that were in contact. Varying degrees of intellectual derivation from the work of earlier diffusionists are clear in a recent and comprehensive symposium largely reflecting this approach (Riley et al. 1971), coupled with a fairly uniform abandonment of the broadly synthetic schemes linking the Old and New Worlds that at one time were confidently put forward. The tracing of Old World–New World interconnections probably remains the dominant theme of this research focus, but reference also may be made to a small number of influential studies more concerned with applying a similar approach within the New World alone (Caldwell 1958, 1964; Ford 1969; Porter 1953). An increased recognition of the problem in demonstrating that technological and stylistic similarities are homologies and not mere analogies may partly explain this attitude of caution, although there is also some indication of a new reluctance to offer reconstructions beyond the strict limits of the evidence. And with regard to evidence, methodological advances are apparent in a greater reliance on the genetics of domesticated plant distributions and on ethnohistoric verifications of (relatively late) contact.

Of much greater relevance for this paper, however, are studies whose repeatedly declared concern is for the "process" and structural impacts of diffusion, rather than for defining formally the bundles of elements that may

have been diffused. As a group, they are not in any way derivative from earlier diffusion studies and concern themselves not with tracing "origins" but with identifying patterns of reciprocal interaction. Many monographs and articles might be cited as illustrative of this approach, among which the following are perhaps representative: Flannery (1968), Winters (1968), Rathje (1971), Struever and Houart (1972), Tourtellot and Sabloff (1972), and Wright (1972). These papers exemplify a highly consistent, currently productive approach that may be characterized by a rather limited series of assumptions, emphases, and propositions:

1. Although sources of particular diffused items, such as obsidian, now can be identified with considerable precision, more concern is directed toward broad patterns of distribution and use of raw materials than toward their precise locus of extraction or the routes by which they moved. Attention is focused (as it generally was not by earlier diffusionists) on specifying the units of time during which relationships may be traced between sites and regions. Thus the establishment of contemporaneity between sites, or of finely divided temporal sequences within them, in most cases becomes a central part of the investigative procedure. Here the continuing technical limitations of a purely archaeological approach to temporal sequence should not be overlooked. A radiocarbon-based dating framework generally still involves a plus-or-minus margin of about two centuries in absolute time (Suess, in Olsson 1970:33), and attempts to translate microstratigraphy into shorter intervals of controlled length are, while promising, still not wholly satisfactory.

2. An analysis of traded or diffused materials is essential. This might be described as a focus on "function" if that term had not been too widely and variously employed for other purposes. Examples include male/female, utilitarian/ceremonial, burial/domestic, and other—generally polar—categories of artifact association and stylistic description. On a somewhat higher plane of abstraction, attempts are made to distinguish subsystems of trade and exchange: interregional vs. local, elite vs. plebeian, political or ritual vs. private, etc. This is accompanied by an emphasis on quantification that sometimes predisposes investigators to aggregate data without regard to possible discontinuities in underlying behavior. Rathje's (1971) assumption that utilitarian names imply task-specific, utilitarian purposes for certain Maya artifacts, which is effectively rebutted by Tourtellot and Sabloff (1972), furnishes a case in point.

3. Although the effort is not as yet marked by any clear agreement as to methodology or independent confirmation of success, some investigators are innovatively groping for means to establish exchange values from purely archaeological data (Winters 1968, Wright 1972). What is involved is a measurement of quantitative relationships of raw materials that, as an intervening step not without large assumptions of its own, can be translated into fixed or recurrent patterns of behavior.

4. Important recognition is given to ecologically based, geographical units of analysis of progressively increasing size and complexity, with different forms and degrees of social interaction characteristic of each. Some investigators (Flannery 1968, Rathje 1972) seek to establish a linkage between ceremonial exchange of "luxury" goods over moderately long distances and the assumed symbiosis in subsistence needs that would have arisen from local environmental crises. More often, however, the researcher is concerned with delineating regional and local resource specialization, without yet being able to take a position on the degree to which such symbiosis might be an explanation. A few investigators (Struever and Houart 1972, Wright

1971) more or less explicitly discriminate levels in a hierarchy of "central places," as an analytical approach to understanding these patterns of economic exchange as well as associated political processes.

At this point it may be appropriate to take a less descriptive and more admonitory stance. Classical central-place theory has as its key proposition a spatial hierarchy of population centers, varying in size with the size of retail market areas. Granting that spatial hierarchies can indeed be traced archaeologically, however, it is questionable how far it is safe to interpret them without establishing the economic-ritual-political-administrative character of the institutions clustered within the centers. As Berry (1967:106) has pointed out, at least in its developed form central-place theory is predicated upon the extreme division of labor and the absence of household self-sufficiency that are characteristic of modern society alone. It also takes for granted the essential autonomy of the mercantile enterprise. But we know that in many cases early trade instead was linked closely with the provision of tribute and labor services to administrative institutions (whether in ritual or political guise), with a redistributive system for the exchange of food surpluses, with the maintenance of a long-distance procurement network for luxuries and necessities of state, or with the provision of agricultural credit from the urban-based merchant and absentee landowner to the rural peasantry. Hence the rationale furnished by central-place theory for the emergence and maintenance of early urban hierarchies may need to be radically revised. What appears to be most useful, therefore, is only the conceptual core of the central-place approach: the idea of progressively larger and more inclusive territories that form a geometrical arrangement and that are associated with some form and degree of increasing settlement scale and sociocultural complexity.

5. In some fundamental respects, most of this research seems to lack a change-oriented, developmental view in spite of numerous programmatic statements to the contrary. It concentrates, for example, upon static, synchronic systems of trade and exchange, making the crucial (although largely implicit) assumption that the idealized "functions" of fully developed systems are adequate to "explain" their origins. This neglect of the transformative mechanisms or processes by which systems arose is likely often to lead to serious teleological distortions.

One illustration is the tendency to infer the full-scale presence of complex institutions or social relationships from formal archaeological traits whose closeness of association with the larger patterns may vary and in any case remains unexamined. On this basis Flannery (1968), for example, argues for symbiosis in subsistence products among multiple coexisting pre-state systems in Pre-Classic Oaxaca. But even if long-distance exchange in ritual or prestige products ultimately may have been a constituent feature or symbolic representation of such a macro-regional pattern of subsistence reciprocity, this could not have occurred until fairly advanced religious and social institutions that could articulate and channel the exchange had emerged, for what then must have been other reasons. Similarly, even though they deal with complex, differentiated social systems in Mesoamerica, Flannery and Rathje (1971) tend to cast their explanations of trade and exchange in terms of undifferentiated, unifying social functions. One also may detect a systematic "gradualist" bias toward slow, irreversible change whose only outcome is a higher, more complex adaptation to a particular ecosystem. Herein lies what is probably the single greatest disjunction between the commonplace assumption of prehistorians and the endlessly reinforced observation of students of contemporary and recent societies.

6. Because of this disjunction, this research shares with most of prehistoric archaeology a selective and inadequate concern for the ethnographic, ethnohistoric, and historic record. Flannery's important article (1968) is a partial exception in its employment of ethnographic case materials, but sometimes seems to rely on comparisons that are too facile. His comparison of the Oaxaca Olmec with highland Burma, for example, overlooks the chronic rice shortages among the Kachin that imposed a permanent subsidiary relationship to the Shan quite unlike anything that can be reasonably suggested for Pre-Classic Oaxaca (Leach 1954:22). Similarly, his comparisons with the Northwest Coast involve a conflation of agricultural and nonagricultural procurement systems and ignore the particularly wide oscillations in yield that were characteristic of the latter (Piddocke 1965).

I do not mean to imply that symbiosis in subsistence products was never an objective behind the exchange of exotic goods. The problem is to determine whether it in fact functioned in this way in a particular case, and with what effectiveness, in view of the high costs of early transport. In seeking means to answer this problem, it is primarily to ethnographic and ethnohistoric materials that we must turn, both for a demonstration of what the constraints of distance were and for suggestions as to additional variables affecting the process. Ford (1972:45), writing of the three-cornered trade between the Comanche, Navajo, and Pueblo Indians during the 19th century, makes the point explicitly after adducing subsistence uncertainties as a vital variable:

For all parties, the constant demand for expendable sacred items maintained a valuable linkage between populations that, in the case of the pueblos, tended toward closed, self-sustaining systems or, in the case of the nomads, gravitated toward alternative means and markets. . . .

Once anthropologists rid themselves of the notion that corn, beans and squash gave sedentary southwestern Indians a lifetime of security, the necessity for these links can be understood. In times of famine when other regulatory mechanisms proved inadequate, contacts, perpetuated by ritual needs, gave the pueblos access to the produce of other ecosystems.

Ford's discussion has another corrective effect upon common anthropological, and especially archaeological, constructs of trade: he illustrates how arbitrary any fixed line of demarcation is between trade and other forms of interactive behavior. To Renfrew (1969:152), for example, trade, even in its "widest sense," involves traffic in commodities "through peaceful human agency." But, as Ford shows, the decision whether to raid or trade not only is often a sudden and spontaneous one but also is subject to intervening gradations from veiled threat to open violence.

7. Closely connected with the absence of sufficient attention to the observed or documented records of non-Western societies is the reflection in this group of articles of the general failure of archaeologists to differentiate between broad social needs and objectives and those of the *agents* of exchange or contact. Similarly, there is essentially no discussion either of the extent of economically motivated entrepreneurship or of the more general role of consciously goal-motivated behavior on the part of elites and specialized minorities. Tourtellot and Sabloff (1972) alone imply their awareness of something of the kind, through their insistence on the autonomy of long-distance trade in response to prestige motivations rather than ecological imperatives.

CONTRIBUTIONS TO A BROADER PERSPECTIVE FROM ETHNOGRAPHY AND ETHNOHISTORY

Raising the question of the agents of exchange and contact—the entrepreneurs—subtly but decisively shifts the

direction of an inquiry into ancient trade. At least part of the attractiveness of central-place theory for archaeologists is that it identifies trade with patterned regularities of the kind they can hope to recover. They are, of course, already familiar with such regularities, and it is probably unavoidable to attach excessive importance to undifferentiated, habitual behavior because of its disproportionate bulk in the evidence with which they deal. But what is usually missing in such evidence is the dynamic or processual element, which can be identified only if we are able to specify the human agents of change in terms of at least some of their positions, roles, traditions, and objectives.

A recent critique of central-place theory by Vance (1970) focuses precisely on the need to identify the initiating, dynamic element. Doubting that the essential autarchy of a region is or has ever been a valid assumption, Vance sees this element in the entrepreneurship of the wholesaler or agent-of-trade. In so doing, he opens the closed hexagons of central-place theory to the major influence of exogenic change. The need for a regular relationship between trade volume and geographic distance, from this viewpoint, diminishes or may disappear altogether. With the invention of writing, transactions are recorded and thus can assume an abstract, differentiated character. Thereafter, they no longer need to be localized in the way that direct exchanges of goods must be. Record keeping also permits (or at least enhances) an accumulation of demand over time, to be discharged by periodic contacts rather than through the enlargement of a permanent marketing center. And in long-distance wholesaling, experience and tradition play a more important role than marketing radius; thus the center-of-initiation may assume an enduring importance quite inexplicable by reference to resource availability or marketing distance.

Vance's empirical concern is primarily with the rather special case (from the anthropologist's viewpoint) of the westward expansion of the United States in the 19th century, but his general findings have obvious relevance for the preindustrial world. There is, moreover, evidence from at least one non-Western setting that has been interpreted in a strikingly similar fashion. Hodder (1965:99-100), reviewing the ethnographic data on markets in East and Central Africa, has found no positive support for the orthodox theory of the endogenous origin of trading centers, noting instead a

remarkable lack of correspondence between the location of traditional periodic markets on the one hand and the location and hierarchy of settlement on the other. Traditional markets here, clearly, are not nuclei of settlement but foci of communications. This phenomenon . . . is at least understandable, even logical, in terms of the theory of market origins which sees markets as being introduced from outside contacts rather than arising naturally within an existing socioeconomic framework.

What is cast into doubt by the work of both Vance and Hodder is the applicability to some—perhaps many—historic settings of the spatial frame of analysis on which central-place theory relies. This in turn casts into doubt any assumed evolutionary sequence in which the growth of a hierarchy of population centers is traced primarily to an individual's propensity to barter surplus goods on a uniformly prevailing, least-effort principle of localized transport. Any such assumption ignores those most generative components of trade that by their nature can overcome the usual inverse relationship between distance and degree of contact. For all of the proclaimed intentions of prehistorians to achieve a "processual" emphasis, we have seen that the limitations of archaeological data generally propel them toward a spatial frame of analysis resting on analogous assumptions. This probably accounts in large part for the failure of most archaeologists to recognize that a meaningful

explanation of the origin and development of systems of interregional exchange is not the same thing as a synchronic account of their subsequent functioning.

A revealing indication of the deep and unexamined difference between archaeological and social anthropological modes of thought is provided by the contrasting ways in which the term "interaction spheres" has been employed. Among North American prehistorians (Caldwell 1964, Struever 1964, Struever and Houart 1972), the term has meant a regionally distinct cultural system that maintains differentiated, long-distance patterns of exchange in exotic materials, stylistic motifs, and, presumably, associated belief systems and ritual. This usage certainly modifies the old archaeological paradigm of "time-space systematics" with new ecological considerations. It also acknowledges the sweeping effects of interregional contact upon some socio-cultural sectors and under certain circumstances. Except in those limited respects, however, it does not really advance very far toward a genuinely "processual" analysis.

Barth's (1967a) "spheres of interaction," on the other hand, are ill-defined spatially and may largely overlap, but they contribute more to the analysis of process and less to a regionally oriented description of raw materials and associated patterns of distributive behavior. Writing of the Sudanese Mountain Fur, he argues that the flow of goods and services is *socially* patterned in discrete spheres; that social barriers exist preventing free movement between these spheres; and that entrepreneurial activity originates and clusters along the barriers in such a way as to take differential advantage of the relatively scarce means by which the barriers may be breached. Phrasing this last point more abstractly, Barth maintains (p. 171) that "entrepreneurs will direct their activity preeminently toward those points in an economic system where the discrepancies of evaluation are greatest, and will attempt to construct bridging transactions which can exploit those discrepancies." Here, it would appear, is a genuinely testable cross-cultural hypothesis that applies to the development of systems of exchange, not merely to their maintenance in an equilibrium system.

To be sure, the details of Barth's analysis illustrate the formidable obstacles that lie in the way of identifying innovative, developmental activity from archaeological sources alone. One economic sphere among the Mountain Fur involves the exchange of labor (in invited labor parties) for home-brewed beer, while another involves "a large variety of material items, including also a monetary medium, and is associated with the market-place facilities." The former is morally the higher, and conversions from the latter to the former are discouraged through "a moral reprobation on conversions from labor to cash and from beer to cash" (p. 156). The successful entrepreneurs recently have been those who find ways to engage beer-paid labor parties in the cultivation of cash crops for market.

Whatever the programmatic aspirations of the "new" archaeology to recover virtually any system of behavior, it is obviously far from being able to identify beliefs and transactions like those that intimately interact to compose the system Barth describes. Most kinds of archival data also would not permit us to identify more than a limited portion of the economic activity in each of these spheres, to assess what their relative magnitudes were, or, above all, to detect where the barriers that separated them lay. But this only means that archaeologists and ancient historians must fashion new methodologies and new subsidiary hypotheses, in order gradually to identify innovative behavior and to record its spread and institutionalization. That they will not soon attain the completeness or persuasiveness

of Barth's account surely does not mean that entrepreneurship can be defined away as a non-problem by exclusive preoccupation with "systemic" behavior, so that they do not even make a beginning.

Contemporary or ethnohistoric sources are likely to be the principal contributors to a more comprehensive, finer-grained analysis, and in particular to a better-balanced appraisal of motivations. Well-documented examples of the impact of trade upon relatively nonsedentary, unstratified peoples are furnished by the Plains Indians, and the general pattern can be briefly illustrated by the findings of Lewis (1942) on the Blackfoot and Jablow (1951) on the Cheyenne. In comparison with the archaeologists we have considered, each attests to a generally more positive view of the transformative effects of trade itself—upon social structure, upon motivation in warfare and tribal leadership, upon preference for material objects securable only through trade, and even upon regional characterizations enshrined as "culture areas."

Lewis's and Jablow's accounts depict a profound alteration in the structure of Plains tribes primarily as a result of their involvement in a trading network. There was a growing dependence on certain categories of trade goods, leading to the disappearance of their native cognates and hence to an even greater, and presently irrevocable, involvement in the commerce in furs. In addition to the demand for the furs themselves, the trade also provided a market for dried provisions, buffalo hides, and horses. In fact, it was only the increasing reliance on the horse as a pack animal that made a large-scale fur industry possible. This in turn greatly increased the economic value of the wives who processed the furs, leading to a new emphasis on polygyny. Age at marriage fell for females and increased for males. Age-grades were borrowed from neighboring groups as a mechanism for expressing and channeling the vertical mobility which accompanied increasing wealth.

Since bridewealth was paid in horses, the key to the acquisition of wealth became success in horse raiding. Hence a shift occurred in patterns of warfare, from large war parties depending upon unified action to small groups with firearms who were no longer responsive to the centralized authority of the war chief. Emphasis on the acquisition of prestige through warfare declined, and individuals even began to boast that they had no coups or scalps to their credit but instead carried on war as a business, for profit in captured horses. The need for horses also served as an important stimulus in the development of the authority of military societies.

At the internal level, then, it appears that widespread polygyny, a tendency toward social stratification, and a shift toward economically acquisitive, venture-taking, individualistic patterns of leadership all can be traced to the influence of trade. But the external manifestations, reflected in the closer articulation of tribes with one another, were equally striking:

Tribal entities became so interdependent on the basis of trade as a method of supplying certain needs which became culturally prescribed and necessary, that it may be possible to conceive of those tribal entities as pursuing technologies which functioned within the larger framework of a trade economy. In terms of this trade economy it becomes possible to visualize a balance of diverse technologies whose production, though not geared to trade in their entirety, is sufficiently interdependent to cause widespread repercussions when any of them are affected by external factors. [Jablow 1951:88–89]

In the past the uniformity of Plains culture has also been attributed to the horse. It was argued that with the greater mobility supplied by the horse there was an increase in inter-tribal contacts and an ensuing spreading of culture traits. The role of the fur trade in stimulating this increasing tribal intercourse has hitherto been neglected. We have shown that tribes who formerly had little

contact with each other met at trading posts, and have suggested that this was probably the manner in which societies of Mandan and Hidatsa were transmitted to the Blackfoot. Radin has pointed to an analogous process which took place in the woodlands, whereby the travelling white fur traders were responsible for much of the uniformity which characterizes the Woodland area, and Goldman has shown that the fur trade was instrumental in the diffusion of Northwest Coast culture elements to the interior. It therefore appears that at least three of our culture areas, the Northwest Coast, the Plains and the Woodlands, are recent historical products due in large measure to the role of the fur trade as an agent of diffusion. [Lewis 1942:61]

The common feature in these accounts, and their significance for the wider study of trade, is the emphasis they attach to a connective network of interaction. We learn that involvement in trade can bring in its wake rapid, massive changes in the structure and technological equipment of a society, as well as in associated patterns of motivation, mobility, and leadership. The net effect is an intensification of the society's interactions with distant as well as neighboring groups. These changes go forward at different rates, of course, and lead to configurations which are neither uniform nor stable.

In such circumstances, to fix upon a particular group (or community, or archaeological "site") as an anthropological isolate can only involve a serious distortion. One can succeed in tracing the growth, coalescence, or disintegration of a group, but the structural corollaries—and hence the "explanation"—of these gross changes are only to be understood by reference to its shifting position in a wider web of relationships. As Leach (1954:60) has eloquently argued for highland Burma, we must eschew the classical approach of drawing the limits of our studies around a locality "of any convenient size" and substitute a regional mosaic whose social segments remain permanently in flux.

Much the same position is articulated in recent studies of more complex societies in Africa. Goody (1971:18–20), for example, speaks of the rapid rise and collapse of numerous small, centralized polities as being facilitated by trade in a matrix of "acephalous" peoples. Kottak's (1972) work on Madagascar similarly suggests that trade alone could disrupt the equilibrium between a group of ecologically specialized, coexisting polities and rapidly elevate a strategically situated chiefdom to the status of a predatory state. The African examples remind us, to be sure, that the Atlantic slave trade and the supply of guns originating in Europe or America provided two powerful, disequilibrating mechanisms that may have had no parallel in earlier antiquity.

Inequalities in access to guns, or simply to the European traders who supplied guns for slaves, certainly could tend to make more permanent a distinction between domination and submission. The drainage of the military manpower of the victimized peoples off into slavery would have had this same effect. At earlier times, the conquering power could have been more easily threatened anew at a time of internal division by any coalition of subjugated peoples. On the other hand, it would be simplistic to suppose that the influence of the demand for slaves and the supply of guns was a permanently stabilizing, culturally uniform one.

The complexity of the political shifts arising from such trade has been illuminated by a recent study by Miller (1970) of the Cokwe expansion in eastern Angola, which began only after the decline of the European market for slaves on the Atlantic coast. Cokwe military preponderance, Miller finds, rested to a considerable degree upon their superior skill in the repair of guns, augmenting their effective firepower. With initial supplies secured by astute

manipulation of the terms-of-trade in the export of wax, they cornered the ivory trade and vigorously expanded in the directions of its receding sources of supply. Well-armed, sophisticated raiding and the incorporation of captured women into the group led to rapid population growth, and conquered lands were initially consolidated by migration into them. But Cokwe politico-military power proved to be tied to their expansion against a populous, resistant enemy. The fragility of the realm became apparent upon the collapse of their Lunde opponents, for that was quickly followed by their own decomposition and subsequent defeat.

While some unprecedented effect of European technological superiority upon the concentration of power probably must be conceded, other agencies in human history surely have had not wholly dissimilar influences. Given the limited role of firearms and the huge preponderance of Indian forces, for example, the role of technology in the long and unbroken series of Spanish conquests in the New World was apparently secondary. Perhaps as a general rule (not without highly contemporary applications) it contributes much less to success in war than military organization—and qualities of military initiative, self-reliance, flexibility in tactics, and morale, for which our earlier rubrics of self-consciousness and goal-motivation may be applicable. Hence it is not wholly surprising to find a recent symposium on African firearms tentatively concluding “that their initial impact was less than had been expected, and that their success in war rapidly declined thereafter” (White 1971:184).

In just this sense, one might look at the formation of the earliest cities as an innovation that was as disequilibrating in its day as the gun and the slave trade were in theirs. What the concentration of population in cities provided was not only a more secure defense of stores of subsistence products, but also the advantage of the greater size, cohesiveness, and discipline of the military units that could be fielded by urban centers than by the more dispersed tribes and principalities around them (Adams and Nissen 1972). A very similar explanation has been proposed by Rosenfeld (1965) for the growth and superiority of the oasis towns of the Nejd at the time of the rise of the Wahabi movement in Arabia during the 19th century.

I am concerned with warfare and urbanization not directly, however, but only insofar as patterns of trade and exchange impinge upon them. In this respect, the question to be asked of the African data is whether the force and ubiquity of European contact so severely distort indigenous trade patterns that the latter cannot be reestablished. The implication of Kottak's discussion is that this is emphatically not so. The same inference can be even more persuasively drawn from a recent volume edited by Meillassoux (1971) and devoted largely to this question.

As this study makes clear, West African trade was of course deeply affected by the special emphasis it acquired on overseas markets for slaves, by the disjunction in technique between African and European traded products, and by the trade stimulus furnished by the prepotency of guns. But the institution of slavery was twofold, providing both the commodities of the Atlantic trade and the producers of most of the goods involved in the intra-African trade, and the latter is shown to be antecedent to effective European influence, extensive, highly diverse in the range of products traded, and crucial with respect to at least some commodities. Another recent survey, by Gray and Birmingham (1970:8–13), comes to exactly the same conclusion for central and eastern Africa. Other studies, such as that of Alpers (1969) on the Yao in Tanzania, demonstrate that the indigenous demand for slaves was an expression of the drive to increase the size and strength

of political units. In that sense, external long-distance trade that was not tied to European influences again emerges as a critical factor in the development of a handful of great trading towns and in the simultaneous suppression of their erstwhile rivals.

Meillassoux's perceptive discussion of the currency problem (p. 70) also reinforces the indigenous aspects of African commerce. We learn that the presence of fungible, aliquot currencies (cowries) was closely tied to the existence of centralized states in which they served as important administrative instruments—for tribute, taxes, and ferry fees as well as for market uses. Because cowries were too bulky to be transported much more economically than the traded commodities themselves, they were generally circulated independently around, and especially within, each major market as a nexus.

Thus, rather than seeing long-distance trade as a European-initiated pattern of integration imposed upon a region that earlier had consisted of autonomous, noninteracting principalities in West Africa, Meillassoux argues for the generative role of *African* trade. This role was independent of and antecedent to the special features it acquired after direct European contact, and helps to account for the emergence of a political geography that included expansive, dominant, centralized polities and dispersed, weak, “acephalous” chiefdoms or other groupings. Although expressed only in terms of a localized application, the general conclusion which emerges has been forcefully and succinctly put forward by Alagoa (1970:319):

... whatever the degree of caution with which the influence of European trade is stated, a fundamental element of exaggeration would remain unless it is equally clearly stated that state structures existed in parts of the Niger delta before the arrival of European traders on the coast of West Africa, and that the new overseas trade was grafted on to an earlier system of long-distance trade within the Niger delta. Indeed this earlier long-distance trade, in all probability, provided the stimulus to the formation of the state systems, in ways similar to the better known changes wrought by the overseas trade.

Also of interest for this discussion is Meillassoux's analysis of the complex, unstable relationship between war and trade. Inasmuch as the commodities that circulated in the intra-African trade were largely produced by slaves, he sees warfare and trade (p. 55) as being “complementary and opposed. The former feeds the second, uses it as an outlet, yet withdraws men from production. Hence two classes develop which are both solidary and antagonistic—a class of warrior aristocrats and a class of merchants.”

The disjunction between these two groups in West Africa was accentuated by the fact that the military aristocracy was generally pagan while the traders were Moslems. This meant that the rivalry was frequently expressed in religious as well as socioeconomic terms. In any case, the traders were not an unresisting complement to the dominant political authorities, gratefully accommodating themselves to the specialized niche that had been left for them. Caravans provided their own security, and at the nodal points in the trading network Moslem saintly communities installed themselves as cohesive, militant nuclei. “Prayer,” as an early European visitor remarked, “is a password.” At the same time, these communities of traders should not be thought of as closed and hostile. Their presence was often actively sought by local chiefs, both for the trade they would attract and for the advantage of the supernatural wisdom that Moslems putatively would bring (Meillassoux 1970:56).

In the case of the trans-Saharan and sub-Saharan caravan trade, its recency permits us to view its complex interrelationships through the eyes of observers with values, pre-

suppositions, and sensitivities relatively close to those of most modern scholars. This is not true for more remote times, even in cases that seemingly are far better recorded in contemporary documents. Hence it is appropriate to consider the ancient Near East, both to draw attention to the limitations of much of the recorded data on early trade (even at its most voluminous) and to provide a comparative perspective to the African case on patterns of commerce between neighboring but unequal polities.

EXTENT AND COMPLEXITY OF EARLY NEAR EASTERN TRADE: HINTS FROM CUNEIFORM SOURCES

The so-called Cappadocian merchant colonies present a classic case of feast and famine—an indigestible glut of information on certain aspects of a complex transactional situation and a dearth of unambiguous evidence as to the wider institutional and geographic setting within which the colonies briefly prospered (for three generations) and then disappeared altogether. There are 15,000 or more tablets from Kültepe (ancient Kanish) alone, but only 3,000 are available even in transliteration—and most of those were pillaged rather than excavated. Still others are known from other colonies at Bogazkoy and Alishar. This sword-of-Damocles of known but unavailable documentation may help to account for the weakness of socioeconomic analyses of the colonies' activities. To borrow a criticism of a different field by Momigliano (1955), the prevailing approaches tend to be unilateral and to be applied "without integration and correction," while "people who do not accept unilateral approaches too often have no approach at all."

One such unilateral approach, that of J. Lewy, involved the supposition that these colonies were outposts and strongpoints in an Old Assyrian Anatolian empire. This supposition is now discredited (cf. Garelli 1963:321–61), but only after long having diverted attention from voluminously recorded economic interrelationships to arguments over a fictional political superstructure. Of more interest, both for Assyriologists and anthropologists, was Polanyi's (1957) foray into the subject. Consistent with his general objective of emphasizing the role of redistributive systems and marketless trading in primitive states, Polanyi offered a sweeping denial—with the Cappadocian colonies as the specific case in point—of the existence of price-fixing, institutionalized markets in the ancient Near East. He argued that a system of ports-of-trade served as a substitute, in which merchants were government functionaries and exchange trade was carried out on the basis of fixed prices (and perhaps fixed quantities) and commissions rather than by independent agents or groups of agents risking their labor and capital for profit.

Polanyi's work may deserve high regard in other respects, but its application to the Assyrian merchant colonies involves major distortions as well as numerous minor errors. Veenhof's (1972) detailed study effectively controverts his whole approach in this particular case and succeeds in demonstrating that the system rested essentially on private venture-taking. This does not exclude the possibility that there were "administered" aspects to the Cappadocian trade, but then there have been such aspects to virtually all trade not idealized in the minds of the Mercantilists.

"Sell at any price" orders (Veenhof 1972:88) highlight the liquidity of the market and hence the risk involved, while carefully recorded calculations of margins of profit and expenses for customs duties and travel costs underline the motivation for gain. Polanyi had argued that silver did not serve at all as an indirect medium of exchange, and that the rationale for the trade was the procurement

of copper for Ashur. It is clear, however, that the archive deals almost not at all with the export of copper, recording instead the highly "capitalistic" procurement of silver and gold in Anatolia in exchange for textiles and a metal generally (but not universally) agreed to be tin. The precious metals then were carried southward to Ashur, where the silver was exchanged for more tin and textiles to be transported northward to the barbarian towns of Anatolia. The interface between economic spheres that supported this entrepreneurial activity (in Barth's sense) was that in Ashur the merchant could obtain 15 shekels of tin for a shekel of silver, while in Anatolia the rate was about 7 to 1. Silver, it can be shown, did indeed function essentially as money for the Assyrian middlemen.

Entrepreneurial activity was by no means confined to this basic pattern of exchange. Smuggling past customs stations maintained by towns along trade routes is well attested. Raw wool, hides, and locally manufactured textiles were vigorously exchanged by these foreign merchants within Anatolia in response to market fluctuations; tin, for example, varied as much as 20% over short periods. Correspondence between husbands in Kanish and their wives, who sometimes assumed charge of textile production in Ashur, attests to their sensitivity to market forces and to intricate problems of credit and liability within partnerships. In short, Polanyi's sweeping denial of a market economy must be flatly refuted for this Cappadocian setting, although Oppenheim's (1957:35) more cautious statement that markets were of limited and marginal importance within Babylonia proper still remains generally consistent with the textual evidence. Even in Babylonia, however, evidence has been adduced for retail merchants whose private activities were carried on in a small *suq* or bazaar rather than in a central marketplace (Veenhof 1972:354).

The more essential point is that the Cappadocian texts illustrate only a limited and arbitrary segment of the trading activity that must be assumed. We learn of the state institutions of Ashur participating with venture capital in some merchant activities. What proportion? With what (if any) administrative control and/or priority in claiming profits? Again, we know from numerous references that many of the textiles sent to Anatolia were "Akkadian." This appears to be an example of what Meillassoux and others have called "relay" trade, with separate transactions and a different set of agents covering their transport from the cities of southern Mesopotamia to Ashur in exchange for silver. Unfortunately, these additional relays are much more difficult to document than the movement of the goods from Ashur into Anatolia.

Some hints of the links in the trading network south of Ashur appear in texts from Larsa discussed by Leemans (1960:77). These record the shipment of "pure silver" from Eshnunna to Larsa, the term being the same as that used for silver sent from Kanish to Ashur. Some of this silver may have been earned by Larsa merchants in local trading at Eshnunna, but Leemans reasonably suggests that in the main it covers the export of goods from Larsa that were sold in Eshnunna. If so, Larsa-to-Eshnunna may constitute another stage in the relay, with the Eshnunna-Ashur stage still to be identified. But the picture was surely more complex and less linear than even these multiple stages suggest, for there are indications that Susa and Sippar were somehow involved with Eshnunna in multilateral arrangements for depot-based trade.

On the basis of an exchange rate for silver three times higher than in Anatolia, at least during the period of the Cappadocian colonies, Leemans argues that the major sources of the copper employed in southern Mesopotamia lay elsewhere. This was the trade with Dilmun (probably modern Bahrain) first presented in detail from the textual

evidence by Oppenheim (1954) and probed archaeologically, with as yet somewhat inconclusive results, by a long series of Danish expeditions (Bibby 1969). Textiles again seem to have been the major items of export from Mesopotamia, supplemented by oil and perhaps silver. Substantial quantities of copper were imported in return, their amounts suggested by vessels engaged in the trade that carried up to ten tons burthen and by customs duties on the individual consignments that approached one ton. In addition, there is mention among the imports of gold, lapis lazuli, pearls (?), certain kinds of wood, and both raw and worked ivory.

Significant as the Dilmun trade undoubtedly was, the immense scale of textile production in relation to known exports suggests that much more remains to be learned of the long-range movement of goods. Waetzoldt's (1972) recent discussion of the Neo-Sumerian textile industry reports a few texts dealing with the procurement by merchants of up to four tons of wool, usually of inferior quality, for shipment and sale in the Gulf. He goes on to note, however, that such records are rather infrequent. By comparison, a single account tablet from the same period totals an amount of wool for processing at Ur in excess of 6,400 tons, and there are indications that almost 9,000 slaves were employed in Ur's textile establishment (Jacobsen 1953:174, 178). Still another text itemizes the composition of a state-administered herd of more than 50,000 sheep, and a Lagash tablet records the transfer to Ur of eight shiploads of wool (Fish 1934:316, 321). Even allowing generously for domestic consumption, the surpluses available for export must have been substantial—far greater than could be consumed by the sparse populations along the Gulf littoral and in the mining areas in Oman. In spite of the high transport costs of overland traffic, a major commitment to caravan traffic—of which the Assyrian outposts in Cappadocia may be only an example—is indicated.

Because of the unavoidably high costs of overland transport, towns intermediate between the mountainous regions that were the sources of strategic raw materials and the lowlands, like Sippar, Eshnunna, and even Ashur, must have been delicately poised in their political relationships as well as their economic ones. The network of interdependence extended from the foot of the Arabian Gulf far into the highlands of Anatolia and Iran. Slight disruptions at any point within that network would affect the entire calculus of cost and profit, decisively (if briefly) redirecting the commercial activities of the major centers. Hence the intermediate centers, dependent on brokerage and transit trade for their very existence, had to be prepared for quick adjustments in the "tilt" of their relationships. A women's weaving establishment in Eshnunna, for example, provided for a working population of 690—surely more than a tenth, perhaps even a fifth, of the entire population of the town (Gelb 1972:3–4). Thus at least a part of the explanation for the volatility of the borderlands between Assyria and Babylonia must derive from these interactions of trade with politics.

Since there is no reason to deny the large-scale mining of Anatolian (as well as Omani) copper, copper and/or bronze must have been exported into surrounding regions where it was absent during the period of the Cappadocian colonies. But as we have seen, southern Mesopotamia was not among the regions normally being supplied from that source because of high transport costs. Yet southern Mesopotamia was engaged in the indirect trade of (Iranian?) tin and textiles for Anatolian precious metals. This only underscores the complexity and scale of the interactive processes—of which we remain mostly ignorant.

If the Cappadocian merchant colonies weren't engaged

in the transport of copper/bronze, who was? By what means of conveyance, since competing caravan commerce does not appear in the Cappadocian letters? Of Veenhof's sample of 3,000 texts—only one-seventh of those excavated—189 deal with some aspect of the caravan trade (1972:69–76). But in that small sample alone, stemming from only the three generations to which the whole archive is limited, we learn of 90 donkey-loads with almost 11 tons of tin being shipped *into* Anatolia. This seems surely excessive for that part of Anatolia's internal consumption met by supplies channeled through Kanish, for the admixture of tin in bronze is likely to have constituted only 5% or less. Hence it would appear to follow that bronze was being alloyed in bulk in Anatolia and shipped thence. In what direction? By whom? In exchange for what? The same 189 texts record the receipt in Anatolia of some 14,500 rectangular bolts of textiles. Were they "consumed" in Anatolia—which, after all, was producing its own coarser varieties—or moved on farther? If the former, the quantities seem excessive. But if the latter, where? By what further "relays"?

In short, the Cappadocian texts, for all their richness as a documentary source, heretofore have allowed us only to glimpse the tip of an iceberg. At best rather inadequate hints are provided as to scale of the trade in certain commodities. It has to be assumed that there were other, many-sided reciprocities in addition to those that are known, involving movements of many other commodities in quantities that as yet one can only surmise. Thus the wider ramifications of the trade still elude us.

On the basis of what is already known, however, there appears to be little doubt that long-distance trade was a formidable socioeconomic force. This was so in spite of its being confined largely to commodities of very high value in relation to weight and bulk because of high transport costs, and in spite of its directly involving only a small part of the population.

Soon after the time of the Cappadocian colonies, it is no surprise to find Hammurabi having to borrow the funds needed to pay his soldiery from powerful merchants, although by this time the latter may have shifted the primary focus of their economic activities away from venture-taking and into usury (Larsen 1967:152). Under Hammurabi's successors, private entrepreneurs even undertook to insure the kingdom of Babylon against fluctuations in the harvests from which the state's income was derived by taxation, a highly remunerative form of usury requiring enormous reserves of capital not similarly tied to the vagaries of the harvest (Koschaker 1942:164–65). This was the natural outcome of processes and policies of economic concentration, of which risk-taking, long-distance trade was both a symptom and for at least a time an essential constituent.

A detailed account of the growth of trade antecedent to the Cappadocian merchant colonies is beyond the scope of this paper (cf. Leemans 1950, 1960). To sketch the process very briefly and perhaps simplistically, the specialist known as the *dam-gar* appears in the mid-3d millennium B.C. At that time he was responsible for intercity procurement, more as an official in an administrative hierarchy than as private venturer. Adding private commissions to these official duties as the demand for luxuries swelled, his activities are thought to have evolved in time into those of the private entrepreneurs we find in Cappadocia. Such, at least, is a "gradualist" reconstruction of the sequence, a reconstruction that minimizes an early entrepreneurial element and places heavy emphasis on the circulation of goods through a state- or temple-administered redistributive system.

The fluidity, extent, and complexity of trade in the time of the Cappadocian colonies suggests, however, that developmental processes leading to the great power and wealth of the merchants unfolded much less smoothly than this "gradualist" view implies. Existing archival sources give disproportionately heavy representation to the administrative activities of the temple and palace. Hence they surely do not depict the *dam-gar*'s broader role, but record only those activities in which he maintained some relationship to the central administrative apparatus. The 32 *dam-gars* and 3 *gal-dam-gars* at a relatively small town like Early Dynastic Shuruppak, for example, seem quite excessive if their involvement in the intercity procurement of goods for the temples or palace was their primary activity (Deimel 1942:5; Leemans 1950:41). And the reference in a Sumerian hymn to Enlil, chief god of the pantheon, as "merchant of the wide earth" (Falkenstein and Von Soden 1953:76) seems inappropriate if the tradition associated with the term was only that of a subordinate, traveling functionary in an administrative hierarchy, rather than an individual who carried on substantial entrepreneurial activities of his own. Again, to know that the *dam-gar* received an allotment of temple land and seemingly was excused from corvée service, or that the more numerous *dam-gars* sometimes were seemingly grouped "under" a *gal-dam-gar* (who elsewhere can be identified as a high temple official), fails to show that the *dam-gar*'s social position or activities were purely those of a temple or state functionary. Alternatively, the *gal-dam-gar* may have been not a supervisor of the others but merely the individual who dealt most closely with the city ruler.

Beyond the activities of the *dam-gars*, the ruling families in Sumerian city-states may well have supplemented the duties of their offices with private venture-taking. Already in Early Dynastic times, reciprocal gifts of copper, donkeys, and other commodities passed between the wives of the *ensis* of Adab and Girsu, with the careful recording of repetitions of the exchange strongly suggesting transactions with commercial overtones rather than merely a politically or ritually inspired exchange of gifts. At that time, around the middle of the 3d millennium B.C., records in Girsu indicate individual shipments of up to ten tons or so of grain and small amounts of other materials, sent to Dilmun to be exchanged primarily for consignments of copper. As Lambert (1953) remarks, the sequence of exchanges suggests that raw materials from the Gulf were passed upstream from town to town along the Euphrates in a series of bilateral transactions, a further example of "relay" trade in which a succession of middlemen interdicted direct contacts between consumers and suppliers.

While the presence of a private entrepreneurial component in formal town-to-town exchanges thus seems fairly likely, an even stronger case can be made for the early role of private commerce in locally produced and distributed commodities like pottery. In the absence of any reference to state supervision of its production and distribution, secular trade in pottery is the only mode of distribution that seems tenable. Similarly, the Bau temple archive from Girsu records the receipt of large quantities of fish from one of its largest groups of subsistence producers but fails to indicate any disbursement as part of the redistributive system. Since a few texts atypical of the archive make clear that there was substantial trade in this commodity—one records a household merchant's receipt of 13,300 processed fish from fishermen (Bauer 1967:382–83), another deals with the private sale of Lagash fish and turtles 140 km. upstream at Nippur (Lambert 1953:65)—the most economical assumption appears to be that the fishermen's deliveries of their product were turned over by the temples to private merchants for marketing.

Finally, mention may be made of the possible economic implications of a body of textual data that has generally been classified only as myth. There is a group of tales recounting journeys of individual gods and goddesses, proceeding from town to town along the Euphrates. Direct testimony is lacking that these pilgrimages were enacted ritually by human parishioners from the cities of which the deities were regarded as resident patrons, but none could be expected in a literary genre allowing only for the appearance of the gods themselves. Hence it is at least a possibility that a cycle of religious festivals regularly moved up and down the major arteries of irrigation and commerce connecting the principal settlements, rather like medieval fairs in Spain or their distant derivative, the mutual visitations of *santos* that survive as far away as Chiapas. Offerings are known to have been carried by a god's ceremonial barge in one such tale (Ferrara 1972), implying a degree of formal reciprocity among divine communities. It is as yet wholly unclear, however, whether such religious occasions actually were accompanied by commercial fairs serving as periodic markets.

This frankly speculative reference to periodic markets recalls the work of Skinner (1964–65) on rural China. An expanding, intensifying network of local periodic markets recently served in that instance as the vehicle for regional economic integration under conditions of relative political stability and steady demographic growth. Was there any Mesopotamian parallel four millennia earlier? If not, to what difference in basic conditions can we trace its absence? The question is worth asking as a source of insight into regularities in human behavior, even if the answer for Mesopotamia is found to be negative. When even the positive evidence is so ambiguous and fragmentary, as I have tried to show, we must be willing to pursue such slender leads if we are ever to advance from describing categories of recorded transactions to reconstructing the institutional context within which they occurred.

CONSEQUENCES FOR CULTURE-HISTORICAL THOUGHT

There are several distinct components to the intellectual setting in which, I have argued, an interest in trade deserves to be intensified and systematized. One is a more adequate acknowledgement by anthropologists of the historic role of consciousness, including the possibility that goal-motivated behavior has been a decisive factor in many social transformations. This is hardly a novel position, to be sure, since it represents one of the dominant directions of Max Weber's massive influence on the social sciences as a whole. But the position is a relatively novel one for archaeology and culture history, for which the reality to be understood still consists of little more than aggregative, unconsciously adaptive behavior.

Perhaps—to venture still a little further in this direction—we have wrongly deprecated the entrepreneurial element in the historical development of at least the more complex societies. Some may argue that this phraseology directs our attention too narrowly to economic elites rather than to frequently more important groups constituted on religious, political, or multiple criteria. But then one must ask how solid the basis is for the anthropologist's traditional denial that individualistic pursuit of economic gain is a general and powerful force outside the range of developed, Western economies as well as in them—a force whose importance is obscured in the archaeological and early historic record only because it did not and could not lead to habitual, repetitive, easily recorded patterns of behavior.

Trade also directs our attention to still another area of

inadequacy in culture-historical, and particularly archaeological, thought. The models of sociocultural development that most influence our thinking are framed in terms of endogenous change. An external event or influence may be thought of as triggering a progressive increase in scale and complexity, but the latter is assumed to have a smoothly unfolding, internal inevitability of its own. Surely, however, it is absurd to think of this as the path that at least the more complex societies have normally followed. They dominate weaker neighbors, coalesce, suffer from varying forms and degrees of predation, develop and break off patterns of symbiosis—all in dizzyingly abrupt shifts. The need to adapt to such shifts probably constitutes, in fact, the single most overwhelming selective pressure to which societies are exposed.

A concern for trade highlights these interactions—partly aggressive, partly symbiotic, at best only partly intelligible to the societies involved, at most times dangerously competitive. As Meillassoux (1971:85) observes, a renewal of interest in trade thus “frees anthropology from its exaggerated concentration on the study of apparently isolated societies resting on institutions which, in this context, appear more ideal than functional.” He rightly emphasizes (p. 86) that the study of trade cannot “be restricted to the examination of an isolated feature, set apart from social and political structures.” And, equally important, “it is clear that trade is not to be treated as a timeless structural phenomenon, but as a continuing process, and often a turbulent, even violent one.”

In the broadest sense, it is this protean character of social institutions like trade—and traders, their individual protagonists—that I wish to emphasize. It is imperative that we consider the extent to which they are at once prey to powerful, uncontrollable forces and themselves the motive forces in rapid, innovative advance and adaptation. Adequate recognition, and still less an adequate explanation, for the frequently decisive importance of this conscious diversity of actions and aspirations will rarely or never be found in the behavioristic gradualism and biological reductionism that are in the ascendancy today among archaeologists, human ecologists, and many culture historians. There are, of course, important problems to which “systemic” approaches offer a most effective key. What is to be resisted is only the belief that the course of transformative development is largely if not exclusively set by aggregative, uniform, unconsciously patterned behavior.

Abstract

Most studies of ancient trade, it is argued, have tended to isolate the subject too narrowly as habitual patterns in the movement of goods. New knowledge continues to be generated using that approach, partly as a result of recent technical advances in archaeology. Substantial further progress, however, will require an understanding of the more complex and multifaceted significance of trade that must be derived mainly from ethnographic and ethnohistoric sources. Trade frequently has been a vital component, for example, in adaptive social responses to risk and uncertainty. Its dynamic effects are suggested by the shifting boundary between trade and intergroup predation. The unstabilizing, generative aspects of trade are, therefore, among its most crucial features. Rather than reflecting broad and undifferentiated sociocultural orientations or patterns, they may stem in large part from the economic entrepreneurship and other goal-motivated behavior of self-conscious individuals and groups. Hence they point

to the need for a less “gradualist” orientation, and correspondingly greater emphasis on exogenous sources of change, in the evolutionary study of early ranked and stratified societies. Examples are adduced in support of this position from the ancient Near East and from North America and Africa at around the time of European contact.

Comments

by J. M. ADOVASIO

Pittsburgh, Pa., U.S.A. 8 III 74

While I am reasonably certain that prehistorians should be indebted to Adams for his thoughtful discussion of ancient trade, I am not altogether sure why. The audience to whom this article is directed would appear to be, to borrow the author's own words, those archaeologists, human ecologists, and culture historians who are steeped in behavioristic gradualism and biological reductionism to the point that they fail to recognize the protean character of trade and traders. The author feels, and probably rightly so, that an adequate explanation of trade or traders cannot be found within a theoretical context in which transformative development is interpreted solely in terms of “aggregative, uniform, unconsciously patterned behavior.” In stressing the conscious, goal-oriented, profit-motivated aspects of trade and traders, Adams is admittedly questioning the current prevalence of models of cultural development which emphasize change of the endogenous variety to the near total exclusion of any other considerations.

As a reductionist of sorts myself, as well as a proponent of endogenous change models, I can fully appreciate having my attention directed toward “exogenous” agencies which influence or cause cultural adjustment, particularly in more complex societies. I would assume that many of my colleagues, who are as inclined as I am to systemic approaches, should likewise benefit from Adams's admonition not to ignore “the conscious diversity of actions and aspirations” in our interpretations of the archaeological record. For his lucid remonstrances in this regard, Adams is to be commended; however, I would appreciate a rather more complete discussion of precisely how “consciousness” as an agent of cultural change is to be abstracted from the archaeological record. As Adams himself notes, “entrepreneurial behavior . . . sometimes eludes identification even by a sophisticated contemporary observer, and can only prove still more elusive . . . in the material remains uncovered by the archaeologist.” Moreover, if, as he states, an advance in analytical techniques is only a necessary and not a sufficient condition for the end of “impressionism” in the archaeological analysis of trade, it is nonetheless a condition with which we *must* inevitably contend. In short, though I concur with the author that recent studies of ancient trade have ignored the economically motivated entrepreneur with the result that he has somehow become lost in the systemic shuffle, I do not think this article (the Cappadocian example notwithstanding) does much in helping us to find him. Whether Adams should be thanked simply for reminding us that this shadowy figure is “there” somewhere or for stimulating us to systematically seek him out is a choice best left to the individual reader.

by BURCHARD BRENTJES

Berlin, G.D.R. 10 III 74

Adams's article leaves the reader with a split impression, but offers a much broader picture than the one-sided

approach of preceding works on the same problem such as Polanyi (1971) or Renfrew (1969).

The necessary polemics against a "diffusionism" in the style of Le Plongeon (1900) or Heyerdahl have led to a reversal of the matter, and now the "anti"-diffusionists take the rigid position, sometimes an ecologism (Binford 1966) and sometimes a no less limited "systemism" (Polanyi 1957). Renfrew's (1969) "evolutionist" scheme is also too one-sided to cover the many aspects of the problem. There is no doubt that ecology and social structures have an important influence on the social activity of men, among other things trade, and that each society has to be studied in its evolution. But neither has the same ecology everywhere and always led to the same consequences (consider, for example, the fundamental differences in the rôle of southern Mesopotamia in the 8th or 3d millennium B.C. and A.D. 1900), nor are social units autonomous, "biological" closed units, nor have all communities developed by themselves all the tools, architectural forms, and other ways of life and culture. Here Adams's point of view is a welcome improvement over previous ones in its acknowledgement of the diverse factors—though it remains quite unexplainable why he returns to the idealism of Max Weber ("goal-motivated behavior").

I believe it to be necessary to enlarge Adams's thesis with a view based on a historical evaluation of all the known facts—that all trading human societies are primarily producing societies, and the distribution and exchange of goods are to be explained in terms of the concrete conditions of the particular social production (including ecology, "diffusion" of new elements, social barriers, historical events, etc.). In the process, it must always be kept in mind that we are dealing with the affairs of thinking and producing men, who may react differently in different concrete situations. An analogous situation by no means leads to the same reaction; there are indeed general historical rules, but we Marxists especially reject determinism in detail, requiring that each case be studied in concreto and not interpreted according to opinion or limited theory.

Thus I also largely accept Adams's concrete analyses (e.g., on Cappadocia), though I miss a search for the historical development of the processes. Polanyi's equation of the absence of market-places in Babylon with the absence of an "economic" market (the exchange of goods according to value) is a simple mistake, considering the present-day trading in *suqs*, e.g., in Cairo. But as far as I can see, everything tends to the idea that the relatively developed trading capitalism of the Cappadocian merchant colonies was a result of late 3d-millennium development and of itself says relatively little about the situation in the city of Aššur. At the same time, the state-institutional ties of the *dam-gar* of the 3d millennium are not to be denied. I think that it was characteristic of Sumerian society to be "closed" and to act as trader mainly abroad. Within the community, the "rationing" system was a nontrading form of distribution. Not until after the dissolution of this system—which seems to have been limited to Sumer—did trade come to flourish within southern Mesopotamian society (that is, beginning with Akkad, for a time controlled in Ur III, but then fully developed in Babylonian times).

I support Adams in his plea for many-sided concrete studies and in his rejection of biased assumptions and welcome his paper for both its critical and its synthetic aspects.

by H. NEVILLE CHITTICK

Aksum, Ethiopia. 7 III 74

I read this article (and write these words) in the course of carrying out excavations at Aksum, in northern Ethiopia.

Much stimulated by Adams's paper, I found myself trying to apply its burden to the problems of the archaeological situation in which I find myself; for the city (and empire) of Aksum is one which I believe to have depended greatly on long-distance trade.

It is true that the questions usually asked by archaeologists about trade are those which they think they might be able to answer. But this surely does not mean that they are necessarily unaware of other questions, or that they think those that they try to answer are in the final analysis the most important. It is, however, usually necessary to answer with some degree of confidence the first questions we pose before we can attempt to assess the social forces exerted by trade or, for example, the reasons a trading centre came into being. In the case of Aksum, I hazard a guess that the capital grew up where it did on the basis of a pre-existing market that may have been at a centre of communication. But this is little more than a guess based on inadequate evidence and will remain so until we have a better idea of the distribution of sites over a wider region, as well as of the nature and volume of trade, all in a diachronic perspective.

Even when we do have the necessary infrastructure, the prospects of being able to put forward views as to the wider social implications of the sort indicated by Adams do not seem to me very good in the case of prehistoric societies, or even literate societies, except when the written sources are comparatively ample. This seems to me to be the case even given the new kinds of archaeological data and hoped-for advances in analytical techniques. (Documentary sources apart, structures built for purposes connected with trade are likely, when they occur, to continue to offer the best evidence in regard to the institutionalization of trade.) But we should certainly bear in mind, and when appropriate emphasize, the very partial picture that from an archaeological point of view we are able to present of the effects of trading—or, indeed, of raiding.

by YEHUDI A. COHEN

New Brunswick, N.J., U.S.A. 26 II 74

Tautology is among anthropology's hallmarks, a consequence of the particularism that, perhaps inevitably, resulted from the ethnographic or archeological study of one locality at a time. The approach of Adams may be regarded as a landmark attempt to break out of the conventional trap in which a unit's culture is used to explain its culture.

Adams has long been in the forefront of those who have regarded archeology as anthropology, stressing that traits and artifacts must be seen as indices of social systems. Here he has gone further and concentrated on what may be regarded as a basic principle in the organization of social life: No unit can survive by relying on the resources of its own immediate habitat; a group's viability depends on the transfer of resources, goods, and skills across its boundaries. This may be carried a step further: Every expansion or contraction in a group's resource base, outside its boundaries as well as inside, alters the degree to which occupational roles depend on resources that are harnessed outside and on the institutional means employed to transfer them across the unit's boundaries. This applies to particular localities as well as total societies. Given the dependence of a unit's organization of social relations on its occupational-role structure, it follows that its organization of social relations depends on the harnessing of resources outside its boundaries and the institutional means employed to transfer them across those boundaries.

Since I have no expertise in the data used by Adams, this is necessarily abstract. It is also the best that can be done within the limits set by the editor; the 300 words

allotted are the extent of the resources established from without. But as Adams has shown, this is an idea whose time has come and will represent one of the next major lines of anthropological inquiry. (Written while spending an hour in line for gasoline, an organized activity made necessary by a contrived or genuine alteration in resources harnessed abroad.)

by ROLF GUNDLACH

Darmstadt, Germany. 10 III 74

The author criticizes the way archaeologists use the term "trade," arguing that for archaeology and culture history "the reality to be understood still consists of little more than aggregative, unconsciously adaptive behavior." Prehistorians and other archaeologists whose only materials are nonwritten ones are known to have difficulties with socioeconomic interpretations. Changes observed in the elements of a culture are likely to be interpreted as a result of an ethnic infiltration or even conquest. Where, however, it is possible to modify or augment archaeological results by the study of written materials, terms such as "trade," "immigration," etc., can be used in explaining archaeological remains only if these terms are correct. The same applies to a conclusion drawn solely from written materials. Therefore, the author has analyzed Sumerian and Old Babylonian data, especially data on the Old Assyrian merchant colonies in Cappadocia.

To supplement his examples, I shall comment on the part trade played in Ancient Egypt. The economy of this state developed without having had much contact with other countries until the middle of the 2d millennium B.C. Even later on, the main components of this economy were still evident. The problem of considering Egyptian trade with neighbouring peoples is not so much that it never lost its character of being managed by the state, but the virtual absence of sources on traders and trading practices (exceptions are ancient reports on special imported goods such as gold from southern or north-eastern neighbours or incense from Punt). To supply Egypt with raw materials, the ancient state had a special instrument: expeditions organized by offices of the state or of a temple. This instrument was not sufficient, however, to satisfy the country's economic needs. As a result, the problem of supplying the nation's wants must have been solved by something like trade, but we know too little about it. Therefore, in making socioeconomic interpretations, we must consider the gaps in our sources.

The author's statements about the part trade plays in changing a culture may be underlined by the fact that trade in Egypt had little effect on cultural change due to the isolation of foreign traders.

by FRANK HOLE

Houston, Tex., U.S.A. 11 III 74

The implications of this important article are so far-reaching that a short comment cannot do justice to them. I shall, therefore, make only one general statement in clarification and then cite some examples which extend Adams's ideas.

As well as an attack on the "straw man of diffusion," one can read Adams's article as a defense of the "Great Man" theory of cultural development in opposition to the "Culturological" viewpoint, or as an attempt to focus on individual factors in particular instances as opposed to societal factors in general instances. The question boils down to whether particular situations have relevance to an understanding of general processes. It appears that Adams would say yes. I would agree and further mention that it is extremely difficult to handle the role of individual initiative

scientifically, if we mean by this what the "new" archeologists do. With this in mind, it is not sufficient to dismiss individual initiative on the grounds that one cannot conceive how to test its effect: there are more factors to consider than meet the archeologist's eye.

Anyone who has had a chance to study population dynamics, agricultural production, and the like must be impressed with major fluctuations and the tendency for statistics to oscillate about a mean. What this means is that in particular instances major corrective ("adaptive") changes that are quite inexplicable when the system is conceived of only as an abstract ideal will take place. When we add to this the role of individuals who take advantage of disequilibrating factors, we are left with situations which can hardly be explained solely with reference to tangible remains.

To the list of situations in which entrepreneurship may arise, I should like to add some from my own experience. In Deh Luran, Iran, the land is owned by a tribe, but for political reasons which are independent of the local scene large numbers of newcomers have found residence in the town. Since there is no land for these people, they have opened up shops and now dominate many aspects of the economy. For them, business is the only means of survival, but the result promises to alter the local system fundamentally as the newcomers gain economic power over the landholders, who have no interest in business.

Another example from Iran concerns nomadic people of low rank and consequently without sufficient land or herds who move into entrepreneurial occupations to ensure their economic survival. By virtue of their marginal status in tribal society, these persons are not constrained to share or to compete in reciprocal exchanges and are thus free to pursue economic interests for their own benefit. Except for its genesis, the situation is similar to what we see in Deh Luran.

I would argue that the motivation for some entrepreneurial activity thus lies in the scarcity of landed resources, which forces some persons out of a traditional livelihood, and in political circumstances which introduce "outsiders" into a system in which they have no traditional rights. In either instance, entrepreneurial activity (which may eventually develop wide ramifications) is an effective solution to the dilemma of survival. It is equally important to note, as Adams has, that such persons, outsiders by origin or status, may be able to bridge cultural boundaries more effectively than "insiders" with vested interests in preserving separation.

Finally, it is worth recalling the situation in Inner Asia. Lattimore (1951) has suggested that at times the Chinese may have fostered trade routes through Mongol territory not so much for the benefit of the Chinese as for a way to "bribe" the nomadic tribes. Thus we should consider providing frontier peoples with luxuries through trade as a possible tool of international politics whose goal is to preserve internal security. Foreign aid today might be seen in the same light.

by WILLIAM H. McNEILL

Chicago, Ill., U.S.A. 8 II 74

Having been shaped by similar intellectual influences, I not surprisingly find myself in full agreement with what Adams has to say about trade and purposive, goal-directed activity as manifested in ancient and modern times among preliterate as well as among literate peoples. It seems to me he ought to give equal (or even superior) weight to raiding and not try to dismiss the role of organized violence

in intergroup relations as he seems eager to do. Indeed, the distinction between trade and raid was frequently unclear in historic times; similar ambiguity must often have prevailed in unrecorded human encounters. Any effort to separate the two activities is more likely to confuse than to clarify thought.

Generally, I suggest that anthropologists are tempted to project upon the deep past the sorts of behavior that prevailed among those human societies that remained small and isolated into modern times. Timidity, withdrawal from disturbing contacts with strangers, and an intense localism obviously conducted to maintenance of conservative modes of life among such peoples. But to take such behavior as typical of the conduct of early human groups and individuals is clearly fallacious, even though many anthropologists, shaped by a tradition that fed upon a romantic distaste for the corruptions of civilized, Western society, are mightily—often unconsciously—tempted to assume such an equation.

Adams challenges this pattern of thought by suggesting that a range of behaviors very like those of modern times existed in preliterate and early literate societies. Surely the assumption of uniformity in the range of human behavior has more to recommend it than any assertion of systematic difference between civilized and uncivilized, rational and nonrational, Western and non-Western modes of conduct.

by JAMES MELLAART

London, England. 4 III 74

Middle Bronze Age trading relations between Anatolia and Mesopotamia fall into two periods: (1) Old Assyrian trade (from sometime in the reign of Erishum through Puzur Assur), with Kültepe II and the interval between II and IB, and (2) the so-called Mari trade (Shamshi-Adad and Zimrilim and possible successors in Syria after the destruction of Mari), with Kültepe IB. The length of this period of active trade is hard to estimate: about $84 + x$ years for Kültepe II, some (estimated) 50 years for the interval between II and IB, and an estimated c. 70 years for Kültepe IB, a total of c. 200 years, or more. Two radiocarbon dates from the palace at Acemhöyük III— 2086 ± 49 B.C. (for the building?) and 1941 ± 58 B.C. (for crabapples, hence the destruction)—yield a range of c. 145 years. This may correspond to Kültepe II, the interval already referred to, and the beginning of IB; the names of Shamshi-Adad and Zimrilim are said to have been found on bullae deposited in the palace before its burning, which should therefore have taken place early in the Kültepe IB period, where Shamshi-Adad's *limus* are likewise recorded.

Unfortunately, for the historian, absolute dates for this period constitute one of the major controversies in Mesopotamian chronology. Even if the Acemhöyük radiocarbon dates are shorn of their tolerance to yield c. 2037 B.C. for the building and c. 1883 B.C. for the burning of the palace early in Kültepe IB, such dates (and I use the calibrated versions) are still at variance with most of the chronologies now in vogue, except that of B. Landsberger, who advocated long ago a date for Shamshi-Adad near to c. 1900 B.C. I introduce this problem here because for too long historians have been content with the traditional adding up of reigns in king lists, drawn up centuries later and not necessarily accurate, while ignoring archaeological evidence as well as the possibilities of radiocarbon dating. On archaeological assumptions, the E.B.3a period at Kültepe is roughly contemporary with the Accadian period, whereas the following Kültepe *karums* IV and III could be contemporary with, respectively, the Ur III period and the earlier Old Assyrian kings from Ilushuma to Erishum.

Unfortunately, Kültepe has not yet yielded inscribed material before *karum* II, although it has been suspected that the foundation of *karum* IV may have been intended to facilitate, perhaps as early as c. 2200 B.C., the beginnings of Mesopotamian trade. Here, then, in Adams's terms, we may have another tip of an iceberg, the base of which archaeological research has not yet revealed, and which may—if Mesopotamian legends are to be trusted—extend back to the Sargonid period.

Of general importance is the fact that but for the preservation of epigraphical material (tablets and seals) and a few figurines and pots, the presence of Assyrian merchants is undetectable in the archaeological record. They lived in Anatolian houses, used Anatolian pottery, in short, adopted local ways of life and did not surround themselves with Assyrian objects. This fact prompts one to consider that traders from distant parts could have been present in many areas and at many times without leaving tell-tale evidence.

The greatest mystery of these trading ventures is the origin of the tin exported in the Old Assyrian period to Anatolia and in the Mari period to Alasiya (Cyprus), Tinayi (location unknown), and Kaptara (Crete). Was the *karum* system, so well established in the independent principalities of Anatolia by the Assyrian state, also employed elsewhere? Our texts are silent on this issue.

Adams justly stresses the fact that the known facets and distribution of the Old Assyrian (and, one should add, the Mari-period) trade are but a fraction of a much wider trade pattern. Local traders lived side by side with the Assyrian merchants in the *karums* of Kültepe, Alişar, Bogazköy (the only three in Anatolia, out of a much larger number, that have yielded tablets—in the case of the two latter, of Kültepe IB date). Beyond the area in which the Assyrian texts of Kültepe II show Assyrian merchants to have been active (the Halys basin and the eastern approaches to northern Mesopotamia), there are a few place names that show some contact with other cultural provinces, e.g., Luhuzatiya (= Lawazantiya) in eastern Cilicia, a possible emporium for Cilician trade. Yet the greater part of southern Anatolia does not figure in the Cappadocian texts, though there is abundant evidence for trading contacts, especially in the later phase, when Central Anatolian influence clearly advanced for the first time into the Ankara region (Karaoğlu, Polatlı) and the fertile Konya plain (Kara Hüyük-Konya). The Central Anatolian pedestalled quatrefoil drinking cup ("*kantharos*"), based on metal prototypes, not only penetrates to Konya and (without its foot) to Beycesultan V in southwestern Anatolia, but is found from Beyşehir to Silifke and appears in a number of variants, in pottery as well as metal, in Crete. The clay sealings from the first palace of Phaistos (MMIB) bear certain resemblances to (as well as differences from) those recovered in a burnt palace at Kara Hüyük-Konya of about the same period (Kültepe IB).

Crete at this period figures in a Mari text as a recipient of tin from Mari, together with Cyprus; Kamares pottery appears in Cyprus, which exports copper to Syria (and Crete?), on the Syrian coast (Ras Shamra, Byblos), and in Egypt. Cypriote wares reach Kültepe IB; Cilician pottery reaches the Konya plain, and Syrian-type cylinder seals occur at Kara Hüyük-Konya. Cremation is introduced there, probably from North Syria, where at Gedikli near Sakcagözü it had been in use (side by side with inhumation) since E.B.3 times. Faience objects appear in Kültepe IB and contemporary Kara Hüyük, and in MMII Crete we have the faience town mosaic of houses, figures, palms, and animals—equally mysterious in origin, for faiences of this period are apparently not yet known in Mesopotamia. Could this technique have come from Egypt? The latter's promi-

nence in trading exchanges is shown by the markedly Aegean features in the jewellery of Princess Khnumet at Dahshur and the numerous silver vessels of probably Cretan origin in the deposit from Tod in the reign of Amenemhet II. The discovery of Egyptian Middle Kingdom statuettes not only in Syria, but also at Knossos and in Anatolia (Adana and Kirikkale), suggests merchants from Egypt carrying their statues with them in case they should die in a foreign land. Egyptian influence is notable in the MMII pottery from Mallia, decorated with a sphinx, cats among trees, and possibly monkeys, unless the latter owe their origin to the monkeys of Thera. Perfume bottles in monkey shape also appear at Phaistos, together with animal-head rhytons which would not seem out of place in Kültepe IB.

Such examples of international trade during this period could easily be multiplied and show that the Cappadocian texts indeed reveal but the tip of an iceberg which involved commercial exchanges throughout the eastern Mediterranean. What is perhaps of even greater importance is that as the result of this trade, literacy and stronger political organisation spread to areas where heretofore palace organisation had not yet evolved (e.g., in Crete). The Assyrian merchants introduced cuneiform writing to the cities of eastern and central Anatolia, and the need for a recording system, although perhaps already in its infancy in southern Anatolia and the Aegean, was strongly boosted. Prototypes for the so-called Hittite Hieroglyphic script appear in Beycesultan VIA and, more developed, on the seals of Kara Hüyük I, Acemköy, and Kültepe IB; in Crete, a hieroglyphic and a linear script evolve independently at Phaistos within MMIB, in this period aptly nicknamed "the age of palaces."

The prosperity of the Middle Bronze Age civilisations evidently excited the greed of many neighbours, who sooner or later managed to disrupt the trade and conquer the erstwhile centers of civilisation; Hurrians, Hittites, and Kassites in Mesopotamia, Kaskaeans and Hittites in Anatolia, Hyksos in Egypt, Mycenaean in Crete. Yet in time an even more intense trading pattern developed—that of the Late Bronze Age, unfortunately not so well documented in the epigraphical record.

by JANICE STARGARDT

Cambridge, England. 5 III 74

From this important paper, I should like to select only two of the many incisive points raised by the author and consider them in relation to an area where very little archaeological research has been done on trade, namely South East Asia.

Adams emphasises at several points the crucial role of trade—itsself a process of interaction—in triggering swift and profound changes in the institutional and economic structure of a society. In relation to trade, he lays special emphasis on the non-habitual, entrepreneurial activities that respond to, or indeed bring about, the flux of economic contacts. He contrasts this attitude with the *gradualist*, *systemic* approach, on the one hand, and, on the other, with the tendency in the work of environmentalists to isolate areas of study.

In the Mekong Delta and in isthmian Thailand, one can distinguish an element of productive tension between the environment, which while favorable to the growth of maritime trade was not overwhelmingly so, and the entrepreneurial activities of early urban centres. In both regions the latter, in seeking to increase their share of sea-borne trade, brought about major modifications in their environment in the form of shipping canals, moated towns, ponds and reservoirs (Malleret 1959-62, Stargardt 1973). Environmentalists might argue, quite rightly, that the develop-

ment of this particular technology would be unthinkable except through long, accurate observation of the movement of monsoon rain over the coastal clays. It is equally true, however, that at Satingpra on the isthmus, the sizeable effort involved in making and maintaining the canals was only sustained for a limited period. We have yet to determine their origin, but they were not maintained after the end of the 13th or the early 14th century. Maritime trade at that time appears to have shifted quite abruptly away from the Songkhla/Satingpra area to Nakorn Sri Thammarat, farther north. Although environmental factors will have played their part in this shift, rival entrepreneurial pressures of peaceful and violent kinds seem to have been decisive.

Adams reminds us of the hiatus that lies between the material object and the processes of social change. Most of our material evidence reflects the results of change; only rarely change in progress. Recent archaeological research in Burma, at Beikthano, should be mentioned in this connection (U Aung Thaw 1968). Beikthano is at present the earliest urban site in South East Asia. The body of archaeological evidence reported, although not presented with either trade and external connections or social change in mind, does lend itself to considerations of this kind (Stargardt 1974). Among much else, the Beikthano material affords new insights into the *Indianisation* of South East Asia, which has been depicted more often as the action of Indian culture on South East Asia than as a complex process of interaction between the two, reinforced at many points over a protracted period.

by BRUCE G. TRIGGER

Montreal, Canada. 22 II 74

Adams again demonstrates his enviable skill in using a specific topic to illuminate the fundamental issues confronting anthropology and to suggest new directions or corrective manoeuvres for our volatile discipline. Since I find myself in broad agreement with what I understand to be Adams's position, I would like to comment on a few of the general issues he raises.

Given the nature of archaeological data, it is not surprising that archaeologists have generally favored a "materialistic" approach and more specifically an ecological one that views culture as man's extrasomatic means of adaptation to his environment. Such a trend was evident in the geographical approach of Cyril Fox and the economic approach of Grahame Clark long before the development of the New Archaeology in the United States. The temptation has been strong to maximize the explanatory power of the ecological approach by assuming that social organization and belief systems are a direct and predictable by-product (epiphenomenon) of ecological adaptation. Any looser and more general claim (e.g., Flannery 1972:424) has the effect of making the ecological approach only one starting point rather than a comprehensive framework for the interpretation of archaeological data (cf. Salisbury [1973:93] on the roles of ecological and decision-making models in economic anthropology).

Through the influence of Leslie White and Julian Steward, a deterministic ecological approach has become dominant in American archaeology in recent years. In social anthropology and other social sciences, this type of approach competes with others that stress the role of man as an actor and a manipulator of his cultural environment. To some degree these two kinds of approaches are complementary and compatible, but this is not inevitably the case. I have recently been attempting to construct a model to

account for the development of traditional northern Iroquoian community structure. In so doing, I have been struck by the inadequacy of traditional ecological explanations (expanding populations, pressure of crucial resources) to explain the adoption of horticulture in the Northeast. On the contrary, the ability of horticulture to realize a general ambition of collecting groups in this area (to lengthen the season during which large summer fishing bands can stay together) seems to be a key component in this process (Trigger n.d.). To taboo psychological explanations because they are currently considered difficult to test is intellectually indefensible.

The closest approaches that archaeologists have made to dealing systematically with conscious behavior are those which view culture as a system of ideas or perceptions. A detailed exposition of this point of view is found in Childe's (1956) little-read *Society and Knowledge*, in which he defines artifacts as "concrete expressions and embodiments of human thoughts and ideas—in a word of knowledge" (p. 1). As the writings of Mao and Sartre also indicate, Marxist intellectuals are by no means ecological or technological determinists. At a more basic level, a concern with ideas and therefore with consciousness is found in Rouse's (1939) concept of mode and some of Deetz's (1967) related work.

Diffusionists likewise have been interested in the processes by which ideas are disseminated, accumulated, and replaced by more efficient alternatives. Underlying the arid debates between diffusionists and independent-inventionists has been a concern with two psychological (rather than historical or ecological) problems: the inventiveness of man and the degree of his psychic unity. Recent more sophisticated studies of diffusion and innovation suggest that archaeology may yet have significant contributions to make to understanding human psychology.

It is easy to object that Adams's concern with the conscious actions and aspirations of specialized groups may be appropriate to text-aided situations but is not a basis from which a text-free (archetypal or ideal?) archaeology can be pursued. If so, text-free archaeology must remain what the New Archaeology has not fully convincingly denied it need be: a fragmentary basis for the study of human behavior. Archaeology has, however, shown surprising ability to draw unexpected inferences from its data. I am therefore hopeful that by drawing upon some of the currently undervalued ideas of Childe, Rouse, and the diffusionists and by paying closer attention to rates of change and to explaining anomalies apparently not covered by ecological explanations it will be possible to learn more about knowledge and values, the conscious manipulation of culture, and the psychological factors that are implicit in such behavior. If progress can be made, the results will complement the achievements of the already successful ecological approach and provide archaeology with the theoretical catholicity appropriate to a major branch of anthropology.

by GARY A. WRIGHT

Albany, N.Y., U.S.A. 12 II 74

Adams's paper raises many important arguments, most of which I agree with. However, 300 words is not enough to elaborate upon all the questions I have, so I'll restrict my remarks to Polanyi.

Polanyi defined "ports of trade" as generally having three features: economic administration, political neutrality, and ease of transportation (e.g., Polanyi and Rotstein 1966:99). But not all were always politically neutral (e.g., Wyndah). Nor were all prices fixed, as there were "free" goods, e.g., in the case of Kanish (Polanyi 1957:20). In trying to make his point, Polanyi stressed (overly so?) what he saw as the

administered aspects, while admitting that there were items whose prices fluctuated. Is Adams overstressing the "capitalistic" ventures to make his? He notes: "This does not exclude the possibility that there were 'administered' aspects to the Cappadocian trade." Polanyi (1957:22–23) noted that private deals occurred, that traders were independent (though they operated under laws), and that they might incur losses. But he was concerned as to how losses might arise. He wrote (p. 22) that "there was no loss on prices" for administered goods.

Adams and Polanyi seem to me often to be discussing different aspects of trade: motivation (Adams) and prices (Polanyi). The question that seems crucial to me for Polanyi's model is whether prices for certain (not all) goods, e.g., tin, were in fact fixed by treaty for long periods of time. Adams notes exchange rates—15 shekels of tin for 1 of silver in Ashur, 7:1 in Anatolia. How were these set? Did they fluctuate freely or by treaty during the three generations of the colony? To what type of goods does the quote "sell at any price" refer? Tin? Or grain, one of the apparently free goods? Only if prices weren't fixed can we say that Polanyi's model for Kanish is wrong.

True, Polanyi did not view silver in this trade as a "medium of exchange" (a term he defined in Dalton 1968:175–203), but as a "standard" and "up to a point, a means of payment" (Polanyi 1957:20). Adams has not shown why "medium of exchange," in Polanyi's terms, is more correct here.

One final comment: How do the different institutions channel motivation? Polanyi's main concerns were with questions such as how production is organized, to what uses resources are put, and how goods are disposed of. The results for different combinations may be a "poor" Melanesian Big-Man or a "rich" American Carnegie. Motivation may be powerful, but it is not necessarily free-wheeling.

by HENRY T. WRIGHT

Ann Arbor, Mich., U.S.A. 13 III 74

In this kaleidoscopic review, Adams advocates the active role of goal-oriented entrepreneurial behavior in trade and hence in rapid cultural advance and adaptation. It is difficult to deny that such behavior is found in all extrasocietal exchange and that some benefit is always sought and must be gained if people are to be motivated to trade. But such a constant feature of the behavior of the human components in exchange arrangements cannot explain the great variability in such arrangements. To explain such cultural phenomena in terms of the attributes of individual personalities is simple reduction.

The first part of the paper is a critique of current approaches. I was surprised to find myself among a "group" we may term the Chicago/Cambridge Eight. To me the differences between the papers of the Eight outweigh the similarities. A few examples must suffice. Adams notes that the papers share a focus on hierarchies of spatial units and implies that this is incorrect, citing Berry's position that central-place theory assumes a differentiated mercantile economy. However, the theory can be derived from simple assumptions, such as effort minimization, which are quite appropriate to early urban society (Garner 1967). In fact, few of the papers use more than the term "central place," while a paper not discussed (Johnson 1972) provides a sober application to problems of early regional organization, as opposed to problems of long-range trade, to which the theory is hardly appropriate. Adams detects a static or "gradualist" bias among the Eight. Some papers are so concerned, but what could be less gradual than the appearance of the Olmec style throughout Central Meso-

america (Flannery 1968) or the explosive rise of protoliterate state and urban systems in Mesopotamia (Wright 1972, Adams and Nissen 1972)? It is easy to criticize a "school" which has been created by bringing together a series of papers marginally related in their reference to a given ontological category but focusing on very different sociocultural problems.

Adams's critique of the group for its failure to engage in ethnographic analogy is hard to accept. Perhaps most members of the Chicago/Cambridge Eight realize that single analogies are most useful in the first states of concept formulation, and that it is subsequently developed conceptual structures that are tested archaeologically. In the constructive second half of his paper, Adams introduces several cases of trade arrangements. The most extensively discussed are those of West Africa in the 18th and 19th centuries A.D. and Greater Mesopotamia in the 19th and 20th centuries B.C. To these might be added that of Mesoamerica in the early 16th century A.D. The similarities of broader institutional context, scale, organization, currencies, and pattern of oscillation in these cases, insofar as such are documented, is remarkable. In each case, however, the crucial understanding of how the flow of goods and currencies was effectively regulated remains unavailable. A detailed comparative study of these phenomena, of the sort of which Adams is a demonstrated master (Adams 1966), would be of great value. A formal conceptualization of such phenomena would necessarily involve the definition of an interrelated set of variables capable of self-maintenance within defined limits. Such a systemic approach has proved useful in the study of such complex phenomena as industrial economies, urban regions, and tribal communities. It would be useful in identifying the stress points in the phenomena which would become important in transformations from one arrangement to another. Certainly the use of such a method is not inherently reduction.

Reply

by R. MCC. ADAMS

Chicago, Ill., U.S.A. 1 iv 74

Most of the commentators seem to be wrestling with approximately the same set of problems as I was in writing this paper, with conspicuously contrary views less in evidence than varying degrees of responsiveness to different underlying philosophical and methodological positions and to the limitations of the available evidence. Hence a crisp series of rejoinders to opposing arguments is not called for. The article went to some (although perhaps not enough) pains, in fact, to indicate that there is no question of replacing present research strategies, but only of seeking to broaden and diversify them.

Cohen, to be sure, goes further, denying that explanations can ever be offered for changes in sociocultural scale and complexity, or perhaps even for a group's continuing viability, within the kinds of ill-defined, enclosing boundaries that we customarily employ. He may be right, although it is not immediately apparent that this polar position is any more adequate as a framework for comprehensive study of change and continuity than the opposite view that has been deeply implanted in the consciousness of archaeologists and cultural ecologists by Julian Steward. The virtue of having two such opposed formulations is, in any case, that a wider range of theoretical alternatives now may guide our empirical investigations. And that is an end that the article also sought to further by more limited means.

Several commentators allude to the article as taking a position on deep and long-standing differences between idealist and materialist conceptions of history. Brentjes, for example, identifies a concern with goal-motivated behavior as an inexplicable return to the idealism of Max Weber. Hole feels it can be read "as a defense of the 'Great Man' theory of cultural development in opposition to the 'Culturological' viewpoint, or as an attempt to focus on individual factors in particular instances as opposed to societal factors in general instances." Apparently this is attractive to Hole at least in the explanation of certain concrete ethnographic and archaeological situations he has encountered, as it clearly is also the basis of a more general, programmatic position articulated by McNeill and Trigger. H. Wright, on the other hand, rebuts my very general criticism that behavioristic gradualism and biological reductionism are in the ascendancy today with the flat statement that an explanation of cultural phenomena in terms of the attributes of individual personalities is itself a form of reductionism.

While I have always operated from a basically materialist standpoint, I freely confess to having had closer and more productive relations with many advocates of opposing views than with many putative allies. Philosophical positions seldom confront one another in pure form, at least not in practical application to concrete problems of historical or sociocultural analysis. If we are to move beyond rigorous, stale reformulations that neither win converts nor permit genuine progress toward understanding highly recalcitrant bodies of data like those with which the article deals, we must seek out new kinds of questions. G. Wright asks the essential one: how do institutions channel motivations? Surely the autonomy of the individual will is never absolute. But that aspirations and perceptions are culturally constrained should not be allowed to obscure the fact that they are also variable. As Barth (1966, 1967) has argued in convincing detail, individuals diverge as they continuously make varying choices among alternative "economizing" strategies, adapting to different circumstances with more or less commonly held systems of constraints and incentives.

In short, I reject an explanation of historical change in terms of individual personality as firmly as does H. Wright. Forms of entrepreneurship, or of goal-motivated behavior more generally, arise within a system of alternative allocations that is culturally determined. But the changes that entrepreneurs may bring about, at least in the short run, arise from an interplay of forces that is never wholly predictable, and that hence is not adequately accounted for as a deterministic succession of forms. Hole's example of the niche for entrepreneurship that "outsiders" are recruited to fill appositely calls our attention to the perils of a doctrine that stresses the culturally imposed uniformities of economic roles at the expense of the range of behavior carried on within them.

Hole's example serves another purpose. Cultural constraints may be greatly relaxed for those cast into interstitial, brokerage, and entrepreneurial roles by the failure of their traditional means of livelihood. The personal success of all but a very few of them is certainly not assured by the heightened variability of their behavior, however, and situational factors are surely a more important determinant of the outcome in most cases than traits of individual personality. Thus a "Great Man" theory, with its stress on individualistic attainments in contradistinction to the patterns of interacting social segments and society at large, was not what was implied by my stress on the role of conscious interest groups in precipitating change.

The point is an important one also in relation to Brentjes's criticism. "Great Men" are in conspicuously short supply in our own time, as are also political and social movements whose long-run performance meets the promise of their initial objectives. Yet a commitment to consciously directed change, in many instances no less significant than unmotivated adaptation under selective pressure, continues all around us. No thoughtful person can have lived through the '60s, for example, without being driven again and again to reflection on the tenacity of revolutionary convictions that sustained the Vietnamese. They struggled, after all, against a military machine whose technological superiority quickly destroyed any "Great Men" who opposed it, and against which even limited victories were only possible after an appalling sacrifice of the individual protagonists. It is probably there, at least in my own case, that the roots lie for a renewed concern with the role of motivation in history.

Space does not permit a full citation of evidence in response to G. Wright's queries on whether the Cappadocian colonies practiced primarily administered trade, as Polanyi claimed, or market trade, as the article argues. Since this question is dealt with specifically and in detail by Veenhof, a brief quotation from the conclusion to the monograph cited in the article may suffice here (Veenhof 1972:399-400):

We can state that in OA (Old Akkadian) trade silver served a purely commercial purpose and functioned as money in all the meanings of the word. . . . The fluctuations of the prices (for tin once within a fairly short period from 13:1 to 16:1, or a difference of more than 20%) do not suggest that prices had been fixed by treaty, as in the system of "treaty-trade," advocated by Polanyi. In general the governments on both sides seem to have played a limited role in the actual trading process, though they checked transactions, imposed taxes, and might even prohibit trade in some articles.

Ambiguities remain, as Wright suggests, and in particular the nature of the archive makes it difficult to assess the impact, if any, on the trade of a variety of possible administrative devices. But that the system is better characterized as a "market" than as a "port of trade" seems assured.

In most other respects, I can only welcome commentators' contributions that extend or clarify my own arguments and areas of competence. Mellaart, in particular, provides a useful discussion of the archaeological evidence not only for the Cappadocian colonies but also for the further ramifications of trade networks of the time into the eastern Mediterranean world. Gundlach reinforces my impression of the difficulties inherent in dealing with trade even from the apparent vantage point of comparatively voluminous state documents. Stargardt, in probing the "productive tension" between the Southeast Asian regional environment and the long-distance trade, reminds us that ecological adaptation, like trade itself, can involve major, conscious, relatively rapid manipulation of the environment. This is also the view that Trigger develops for the Iroquois. Although I was not aware of trying "to dismiss the role of organized violence in intergroup relations," I am indebted to McNeill for calling attention to this as a possible ambiguity.

It is difficult to respond to H. Wright's vigorous defense of the Chicago/Cambridge Eight "school," since I merely sought to trace the strands of consistency in a group of papers chosen as representing the current, and on balance concededly productive, approach to the archaeological study of trade. Certainly no one who knows the particular eight authors to whom reference is made would think of them as constituting a "school," and the term is Wright's, not mine. A case could no doubt be made for many different groups of scholars and publications, but I see no reason

to alter the characterization of common trends of thought that the article offers. Perhaps it is a matter of distance from the subject or of desired plane of abstraction, with Wright especially conscious of differences among his fellow archaeologists while I, writing for a broader audience that I hope will include some social anthropologists and ethno-historians, tended to stress their common outlook. His defense, in any case, embraces certain positions that were not under attack. My discussion of central-place theory, for example, specifies apparent limitations but concludes with an assessment of the utility of its conceptual core that is very similar to his own.

Adovasio's is the archaeological reaction that I had expected with greater force (or expression of puzzlement), and from more numerous commentators. But while I, like him, "would appreciate a rather more complete discussion of precisely how 'consciousness' as an agent of cultural change is to be abstracted from the archaeological record," at this writing I am certainly no better able than he to provide such a discussion. Perhaps, if we merely await a few more methodological refinements, it will fall within the claim of L. R. Binford and his followers ultimately to be able to recover "the fossilized structure of the total cultural system" (Binford 1972:136). Mellaart's comment that "the presence of Assyrian merchants is undetectable in the archaeological record" of the Cappadocian *harums* is a powerful reminder, however, that we'd better be prepared to give the Binfordians a little time on this one. Chittick reinforces the same pessimistic view, with his expression of doubt as to our abilities to recover "the wider social implications" except where the archaeological record can be substantially supplemented by documentary sources.

The real issue here, however, is less one of the potentialities of methodology than of epistemology. Much of the time, archaeologists can best advance the state of their science by making small, ideally cumulative extensions in the borders of the known and by dealing with their field only in terms of concepts that are currently testable—but not all of the time for all archaeologists, or perhaps even for any archaeologist. It would be a mistake, I would argue strongly, ever to let a current set of instruments of analysis be transformed into a closed system of explanation.

References Cited

- ADAMS, ROBERT McC. 1966. *The evolution of urban society*. Chicago: Aldine. [HTW]
- ADAMS, ROBERT McC., and HANS J. NISSEN. 1972. *The Uruk countryside: The natural setting of urban societies*. Chicago: University of Chicago Press.
- ALAGOA, E. J. 1970. Long-distance trade and states in the Niger delta. *Journal of African History* 11:319-29.
- ALPERS, EDWARD A. 1969. Trade, state, and society among the Yao in the nineteenth century. *Journal of African History* 10:405-20.
- BARTH, F. 1966. *Models of social organization*. Royal Anthropological Institute, Occasional Paper 23.
- . 1967a. "Economic spheres in Darfur," in *Themes in economic anthropology*. Edited by R. Firth, pp. 149-74. London: Tavistock.
- . 1967b. On the study of social change. *American Anthropologist* 69:661-69.
- BAUER, JOSEF. 1967. *Altsumerische Wirtschaftstexte aus Lagasch*. Unpublished Ph.D. thesis, Julius-Maximilians-Universität, Würzburg, Germany.
- BERRY, BRIAN J. L. 1967. *Geography of market centers and retail distribution*. Englewood Cliffs: Prentice-Hall.
- BIBBY, GEOFFREY. 1969. *Looking for Dilmun*. New York: Knopf.
- BINFORD, LEWIS R. 1964. A consideration of archaeological research design. *American Antiquity* 29:425-41.
- . 1972. *An archaeological perspective*. New York and London: Seminar Press.
- CALDWELL, JOSEPH R. 1958. *Trend and tradition in the prehistory of the eastern United States*. American Anthropological Association Memoir 88.

1964. "Interaction spheres in prehistory," in *Hopewellian studies*. Edited by J. R. Caldwell and R. R. Hall. Illinois State Museum Scientific Papers 12.
- CHILDE, V. G. 1956. *Society and knowledge*. New York: Harper. [BGT]
- DALTON, GEORGE. 1968. *Primitive, archaic and modern economies: Essays of Karl Polanyi*. New York: Doubleday Anchor. [GAW]
- DAVID, PAUL A. 1966. "The mechanization of reaping in the ante-bellum Midwest," in *Industrialization in two systems: Essays in honor of Alexander Gerschenkron*. Edited by H. Rosovsky, pp. 3-39. New York: Wiley.
- DEETZ, JAMES. 1967. *Invitation to archaeology*. New York: Natural History Press. [BGT]
- DEIMEL, A. 1924. *Wirtschaftstexte aus Fara*. Wissenschaftliche Veröffentlichungen der Deutschen Orientgesellschaft 45.
- ERASMUS, C. J. 1969. Explanation and reconstruction in cultural evolution. *Sociologus* 19:20-38.
- FALKENSTEIN, ADAM, and WOLFRAM VON SODEN. 1953. *Sumerische und akkadische Hymnen und Gebete*. Zurich: Artemis Verlag.
- FERRARA, A. J. 1972. The itinerary of Nanna-Suen's journey to Nippur. *Orientalia* 41:1-4.
- FISH, T. 1934. Aspects of Sumerian civilization during the third Dynasty of Ur II: About wool. *Bulletin of the John Rylands Library* 18:315-24.
- FLANNERY, KENT V. 1968. "The Olmec and the Valley of Oaxaca: A model for inter-regional interaction in Formative times," in *Dumbarton Oaks conference on the Olmec*. Edited by Elizabeth P. Benson, pp. 79-110. Washington: Dumbarton Oaks Research Library.
1972. The cultural evolution of civilizations. *Annual Review of Ecology and Systematics* 3:399-426. [BGT]
- FORD, JAMES A. 1969. *A comparison of Formative cultures in the Americas*. Smithsonian Contributions to Anthropology 11.
- FORD, RICHARD I. 1972. "Barter, gift, or violence: An analysis of Tewa intertribal exchange," in *Social exchange and interaction*. Edited by E. N. Wilmsen, pp. 2-45. Museum of Anthropology, University of Michigan, Anthropological Papers 46.
- GARELLI, PAUL. 1963. *Les Assyriens en Cappadoce*. Bibliothèque Archéologique et Historique de l'Institut Français d'Archéologie d'Istanbul 19.
- GARNER, B. 1967. "Models of urban geography and settlement location," in *Models in geography*. Edited by Richard Chorley and Peter Haggett. London: Methuen. [HTW]
- GELB, I. J. 1972. The Arua institution. *Revue d'Assyriologie* 66:1-32.
- GOODY, JACK. 1971. *Technology, tradition, and the state in Africa*. London: Oxford University Press.
- GRAY, R., and D. BIRMINGHAM. Editors. 1970. *Pre-colonial African trade*. London: Oxford University Press.
- GRILICHES, ZVI. 1960. Hybrid corn and the economics of innovation. *Science* 132:275-80.
- HÄGERSTRAND, TORSTEN. 1967. *Innovation diffusion as a spatial process*. Chicago: University of Chicago Press.
- HODDER, B. W. 1965. Some comments on the origins of traditional markets in Africa south of the Sahara. *Institute of British Geographers, Transactions* 36:97-105.
- JABLOW, JOSEPH. 1951. *The Cheyenne in Plains Indian trade relations 1795-1840*. American Ethnological Society Monograph 19.
- JACOBSEN, THORKILD. 1953. "On the textile industry at Ur under Ibbi-Sin," in *Studia Orientalia Ioanni Pedersen*, pp. 172-87. Helsinki: Hauniae.
- JOHNSON, GREGORY A. 1972. "A test of the utility of central place theory in archaeology," in *Man, settlement and urbanism*. Edited by Peter Ucko, Ruth Tringham, and G. W. Dimbleby, pp. 769-85. London: Duckworth. [HTW]
- KOSCHAKER, PAUL. 1942. Zur Staatlichen Wirtschaftsverwaltung in Altbabylonischer Zeit, insbesondere nach Urkunden aus Larsa. *Zeitschrift für Assyriologie* 47:135-80.
- KOTTAK, CONRAD P. "A cultural adaptive approach to Malagasy political organization," in *Social exchange and interaction*. Edited by E. N. Wilmsen, pp. 107-28. Museum of Anthropology, University of Michigan, Anthropological Papers 46.
- LAMBERT, M. 1953. Textes commerciaux de Lagash (Époque Pré-sargonique). *Revue d'Assyriologie* 47:57-69, 105-20.
- LARSEN, MOGENS T. 1967. *Old Assyrian caravan procedures*. Nederlands Historisch-Archaeologisch Instituut te Istanbul, Uitgaven 22.
- LATTIMORE, OWEN. 1951. *Inner Asian frontiers of China*. American Geographical Society Research Series 21. [FH]
- LEACH, EDMUND R. 1954. *Political systems of highland Burma*. London.
- LEEMANS, W. F. 1950. *The Old Babylonian merchant: His business and his social position*. Leiden: Brill.
1960. *Foreign trade in the Old Babylonian period*. Leiden: Brill.
- LE PLONGEON, AUGUSTE. 1900. *Queen Moo and the Egyptian Sphinx*. New York.
- LEWIS, OSCAR. 1942. *The effects of white contact upon Blackfoot culture, with special reference to the role of the fur trade*. American Ethnological Society Monograph 6.
- LONGACRE, WILLIAM A. Editor. 1970. *Reconstructing prehistoric Pueblo societies*. Albuquerque: University of New Mexico Press.
- MALLERET, L. 1959-62. *L'archéologie du delta du Mékong*. École française d'extrême orient.
- MEILLASSOUX, CLAUDE. Editor. 1971. *The development of indigenous trade and markets in West Africa*. London: Oxford University Press.
- MILLER, JOSEPH C. 1970. "Cokwe trade and conquest in the nineteenth century," in *Pre-colonial African trade*. Edited by R. Gray and D. Birmingham, pp. 174-201. London: Oxford University Press.
- MOMIGLIANO, A. 1955. "George Grote and the study of Greek history," in *Contributo alla storia degli studi classici*, pp. 213-31.
- OLSSON, I. U. Editor. 1970. *Radiocarbon variations and absolute chronology*. (Nobel Symposium 12.) Stockholm: Almqvist and Wiksell; New York: Wiley Interscience Division.
- OPPENHEIM, A. LEO. 1954. The seafaring merchants of Ur. *Journal of the American Oriental Society* 74:6-17.
1957. "A bird's-eye view of Mesopotamian economic history," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson, pp. 27-37. Glencoe: Free Press and Falcon's Wing Press.
- PARSONS, LEE A., and BARBARA J. PRICE. 1971. "Mesoamerican trade and its role in the emergence of civilization," in *Observations on the emergence of civilization in Mesoamerica*. Edited by R. F. Heizer and J. A. Graham, pp. 169-95. Contributions of the University of California Archaeological Research Facility 11.
- PIDDOCKE, STUART. 1965. The potlatch system of the southern Kwakiutl: A new perspective. *Southwestern Journal of Anthropology* 21:244-64.
- POLANYI, KARL. 1957. "Marketless trading in Hammurabi's time," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson, pp. 12-26. Glencoe: Free Press and Falcon's Wing Press.
- POLANYI, KARL, and A. ROTSTEIN. 1966. *Dahomey and the slave trade: An analysis of an archaic economy*. Seattle: University of Washington Press. [GAW]
- PORTER, MURIEL N. 1953. *Tlatilco and the Pre-Classic cultures of the New World*. Viking Fund Publications in Anthropology 19.
- RATHJE, WILLIAM I. 1971. The origin and development of lowland Classic Maya civilization. *American Antiquity* 36:275-85.
- RENFREW, COLIN. 1969. Trade and culture process in European prehistory. *CURRENT ANTHROPOLOGY* 10:151-60.
1970. New configurations in Old World archaeology. *World Archaeology* 2:199-211.
- RILEY, CARROLL J., J. C. KELLEY, C. W. PENNINGTON, and ROBERT L. RANDE. Editors. 1971. *Man across the sea: Problems of pre-Columbian contacts*. Austin: University of Texas Press.
- ROSENFELD, HENRY. 1965. The social composition of the military in the process of state formation in the Arabian Desert. *Journal of the Royal Anthropological Institute* 95:75-86, 174-94.
- ROUSE, I. 1939. *Prehistory in Haiti: A study in method*. Yale University Publications in Anthropology 21. [BGT]
- SALISBURY, RICHARD F. 1968. "Trade and markets," in *International Encyclopedia of the Social Sciences*, vol. 16, pp. 118-22. New York: Macmillan and Free Press.
1973. Economic anthropology. *Annual Review of Anthropology* 2:85-94. [BGT]
- SKINNER, G. WILLIAM. 1964-65. Marketing and social structure in rural China. *Journal of Asian Studies* 24:3-43, 195-228, 363-99.
- STARGARDT, J. 1973. Southern Thai waterways: New archaeological evidence on agriculture, shipping and trade. *Man* 8:(1):5-29. [JS]
1974. The oldest monumental site in South East Asia. *Antiquity*. In press.
- STEWART, JULIAN H. 1955. *Theory of culture change*. Urbana: University of Illinois Press.
- STRUEVER, STUART. 1964. "The Hopewell interaction sphere in riverine-western Great Lakes culture history," in *Hopewellian studies*. Edited by J. R. Caldwell and R. R. Hall, pp. 85-106. Illinois State Museum Scientific Papers 12.
- STRUEVER, STUART, and GAIL L. HOUART. 1972. "An analysis of the Hopewell interaction sphere," in *Social exchange and interaction*. Edited by E. N. Wilmsen, pp. 47-79. Museum of Anthropology, University of Michigan, Anthropological Papers 46.
- TOURTELOT, GAIR, and JEREMY A. SABLOFF. 1972. Exchange systems among the ancient Maya. *American Antiquity* 37:126-35.
- TRIGGER, BRUCE G. n.d. The children of Aataentsic: A history of the Huron people to 1660. MS. [BGT]
- U AUNG THAW. 1968. *Beikthano excavation report*. Rangoon: Government Printing Office.
- VANCE, JAMES E., JR. 1970. *The merchant's world: The geography of*

- wholesaling*. Englewood Cliffs: Prentice-Hall.
- VEENHOF, K. R. 1972. *Aspects of Old Assyrian trade and its terminology*. Studia et Documenta ad Iura Orientis Antiqui Pertinentia 10.
- WAETZOLDT, H. 1972. *Untersuchungen zur neusumerischen Textilindustrie*. Centro per le Antichità e la Storia dell'Arte del Vicino Oriente, Studi Economici e Tecnologici 1. Rome.
- WHITE, GAVIN. 1971. Firearms in Africa: An introduction. *Journal of African History* 12:173–84.
- WILLEY, GORDON R. 1971. Review of: *A comparison of Formative cultures in the Americas*, by J. A. Ford (Smithsonian Contributions to Anthropology 11, 1969). *American Journal of Archaeology* 75:117.
- WINTERS, HOWARD D. 1968. "Value systems and trade cycles of the late Archaic in the Midwest," in *New perspectives in archaeology*. Edited by S. R. Binford and L. R. Binford, pp. 175–222. Chicago: Aldine.
- WRIGHT, HENRY T. 1972. "A consideration of interregional exchange in greater Mesopotamia: 4000–3000 B.C.," in *Social exchange and interaction*. Edited by E. N. Wilmsen, pp. 95–105. Museum of Anthropology, University of Michigan, Anthropological Papers 46.